PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCX.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

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be, the importance and singularity of the subjects, or the advantageous manner of treating them, without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1809, to Mr. EDWARD TROUGHTON, for the Account of his Method of Dividing Astronomical Instruments, printed in the last Volume of the Philosophical Transactions.

PHILOSOPHICAL TRANSACTIONS.

I. *The Croonian Lecture.* *By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read November 16, 1809.

I AM aware that the remarks, which I have to offer on the present occasion, may be thought to bear too little direct relation to each other for insertion in the same lecture; yet any observation respecting the mode of action of voluntary muscles, and every enquiry into the causes which derange, and into the means of assisting the action of the heart and blood-vessels, must be allowed to promote the design of Dr. CROONE, who instituted these annual disquisitions. And it has always appeared to be one great advantage attending the labours of this Society, that it favours the production of any original knowledge, however small, in a detached form; and enables a writer to say all that he knows upon a particular subject, without inducing him to aim at the importance of a long dissertation.

I shall therefore make no apology for dividing the following lecture into three distinct parts.

In the first of which I shall treat of the duration of voluntary action.

In the second, I shall attempt to investigate the origin of sea-sickness, as arising from a simple mechanical cause deranging the circulation of the blood

In the third, I shall endeavour to explain the advantage derived from riding and other modes of gestation, in assisting the health under various circumstances, in preference to every species of actual exertion.

Part I. *On the Duration of Muscular Action.*

THE necessity of occasional intermissions from a series of laborious exertions, is within the experience of every one; the fatigue of continuing the effort of any one voluntary muscle without intermission even for a few minutes is also sufficiently known; but there is a third view of the duration of muscular action which appears to have escaped the notice of physiologists; for I believe it has not hitherto been observed that each effort, apparently single, consists in reality of a great number of contractions repeated at extremely short intervals: so short indeed that the intermediate relaxation cannot be visible, unless prolonged beyond the usual limits by a state of partial or general debility.

I have been led to infer the existence of these alternate motions from a sensation perceptible upon inserting the extremity of the finger into the ear. A sound is then perceived which resembles most nearly that of carriages at a great distance passing rapidly over a pavement.

The rapidity of the motion varies according to the degree of force with which the finger is retained in its place. The sound thus perceived is not at all dependent on the degree of pressure upon the tympanum; for, on the contrary, the vibratory sound is most distinct when that pressure is slight, if the finger be at the same time rendered rigid by the forcible action of antagonist muscles; and when the ear is stopped with great force without the presence of muscular action, no such sound is produced. For instance, if the head be rested upon the hand in such a position, as to press with its whole weight upon the ball of the thumb applied to the ear, no noise is perceived, unless the extremity of the thumb be at the same time pressed against the head, or unless the action of some other muscles be communicated to the ear, by any inadvertence in the method of conducting the experiment.

When I endeavoured to estimate the frequency of these vibratory alternations, they appeared to be in general between 80 and 90 in a second; but it is possible that the method I employed may be found defective, and it is to be hoped that my estimate may be corrected, by some means better adapted to the determination of intervals that cannot actually be measured.

It was by imitation alone that I was enabled to judge of their frequency. For this purpose I contrived to render the vibration itself, and the imitative sound, both audible by the same ear.

While my ear rested on the ball of my thumb, my elbow was supported by a board lying horizontally, in which were cut a number of notches of equal size, and about $\frac{1}{8}$ of an inch asunder. Then, by rubbing a pencil or other round piece of

wood with a regular motion along the notches, I could imitate pretty correctly the tremor produced by the pressure of my thumb against my head, and by marks to indicate the number of notches passed over in 5 or 10 seconds, observed by my watch, I found repeated observations agree with each other as nearly as could be expected; for I could not depend upon exerting the same degree of force in different trials.

That I might not be deceived by the resemblance of tremors, which coincided only at alternate beats, and therefore might be considered as octaves in music to each other, I sometimes employed notches at greater and sometimes at less distances from each other, but the result was nevertheless the same; and in order to avoid any error that might be caused by some accidental quality of the sound arising from the length of the muscle employed, or length of the bones concerned in conveying the imitative sound to my ear, I made the following variation of the experiment. My ear was stopped by a cushion pressed upon by the end of a notched stick that rested on my foot, and thus conveyed the vibration from the muscles of my leg to the ear, along with the tremor produced by friction upon the notches; and still the results were nearly the same; varying in frequency between 20 and 30 in a second, according to the degree of force exerted in the experiment.*

* The resemblance of the muscular vibrations to the sound of carriages at a distance, I apprehend to arise not so much from the quality of the sound as from an agreement in frequency with an average of the tremors usually produced by the number of stones in the regular pavement of London, passed over by carriages moving quickly.

If the number of vibrations be supposed 24 in a second, and the breadth of each stone be about 6 inches, the rate of a carriage thus estimated would be about 8 miles

As a further proof that I was not much deceived in my judgment of the frequency of these vibrations, I requested two or three of my friends to repeat the same experiment for me, and our agreement was such as to confirm me in the opinion, that there could be no very considerable error in the estimate.

The greatest frequency that I think I have observed, was about 85 or 86 in a second, and the least was as low as 14 or 15; but in attempting to lessen the number of vibrations, there appears to be a degree of unsteadiness which prevents any accurate measurement of the real number.

It is very probable, that in cases of great debility the number may be even considerably less, and may be the reason of that visible unsteadiness, which is known to occur in persons enfeebled by age, or much reduced by disease.

Possibly the foregoing observation may not be altogether new to some members of this Society, as it is now about 17 or 18 years since it first occurred to me, and I was then accustomed occasionally to mention it in conversation with my friends; but I am not aware that any other person has made the same remark respecting the vibratory nature of muscular action, although I find that Grimaldi had observed the sound that occurs upon stopping the ears, but ascribed it, according to the notions that prevailed in his time, to the hurried motions of the animal spirits.*

an hour, which agrees with the truth as nearly as the assumptions on which the estimate is founded.

* Vera itaque ratio experimenti prædicti est, quia in digito et brachio totoque corpore continuato fiunt multi motus ac tremores, ob spirituum agitationem huc illuc perpetuo accurrentium. GRIMALDI, Physicomathesis de Lumine, p. 383.

Part II. *On Sea-Sickness.*

THE second remark which I have to offer to the Society relates to sea-sickness, the cause of which has not hitherto been fully explained; and although the explanation which I am about to propose, may not appear altogether satisfactory to persons who, when at sea, are also rendered giddy by the incessant motion of the waves, and are consequently liable to consider as cause and effect phænomena which in their minds are constantly associated, yet the observation on which it is founded may deserve to be recorded, on account of the degree of relief that may be obtained in that most distressing affection.

After I had been harassed by sea-sickness during a short voyage for some days, and had in vain attempted to account for the difference between the inexperienced passenger, and those around him more accustomed to the motion of the sea, I imperceptibly acquired some power of resisting its effects, and had the good fortune to observe a peculiarity in my mode of respiration, evidently connected with the motion of the vessel, but of which, in my then enfeebled state, I was unable to investigate either the cause or consequence. In waking from a state of very disturbed sleep, I found that my respirations were not taken with the accustomed uniformity, but were interrupted by irregular pauses, with an appearance of watching for some favourable opportunity for making the succeeding effort; and it seemed as if the act of inspiration were in some manner to be guided by the tendency of the vessel to pitch with an uneasy motion.

The mode by which I afterwards conceived that this action

could primarily affect the system, was by its influence on the motion of the blood; for, at the same instant that the chest is dilated for the reception of air, its vessels become also more open to the reception of the blood, so that the return of blood from the head is more free than at any other period of a complete respiration. On the contrary, by the act of expelling air from the lungs, the ingress of blood is so far obstructed, that, when the surface of the brain is exposed by the trepan, a successive turgescence and subsidence of the brain is seen, in alternate motion with the different states of the chest. It is probably from this cause that, in severe head-aches, a degree of temporary relief is obtained by occasional complete inspirations.

In sea-sickness also the act of inspiration will have some tendency to relieve, if regulated so as to counteract any temporary pressure of blood upon the brain; but the cause of such pressure requires first to be investigated.

All those who have ever suffered from sea-sickness (without being giddy) will agree that the principal uneasiness is felt during the subsidence of the vessel by the sinking of the wave on which it rests. It is during this subsidence that the blood has a tendency to press with unusual force upon the brain.

If a person be supposed standing erect upon deck, it is evident that the brain, which is uppermost, then sustains no pressure from the mere weight of the blood, and that the vessels of the feet and lower parts of the body must contract, with a force sufficient to resist the pressure of a column of blood, of between five and six feet from the head downwards.

If the deck were by any means, suddenly and entirely

removed, the blood would be no longer supported by its vessels; but both would fall together with the same velocity by the free action of gravity; and the same contraction of the vessels which before supported the weight of the blood would now occasion it to press upon the brain, with a force proportional to its former altitude.

In the same manner, and for the same reason, during a more gradual subsidence of the deck, and partial removal of support, there must be a partial diminution of the pressure of the blood upon its vessels, and consequently, a partial reaction upon the brain, which would be directly counteracted by a full inspiration.

The consequence of external motion upon the blood will be best elucidated by what may be seen to occur in a column of mercury similarly circumstanced.

A barometer, when carried out to sea in a calm, rests at the same height at which it would stand on shore; but, when the ship falls by subsidence of the wave, the mercury is seen apparently to rise in the tube that contains it, because a portion of its gravity is then employed in occasioning its descent along with the vessel; and, accordingly, if it were confined in a tube closed at bottom, it would no longer press with its whole weight upon the lower end. In the same manner, and for the same reason, the blood no longer presses downwards with its whole weight, and will be driven upwards, by the elasticity which before was merely sufficient to support it.

The sickness occasioned by swinging is evidently from the same causes as sea-sickness, and that direction of the motion which occasions the most piercing sensation of uneasiness, is conformable to the explanation above given.

It is in descending forwards that this sensation is perceived; for, then the blood has the greatest tendency to move from the feet towards the head, since the line joining them is in the direction of the motion. But when, in the descent backwards, the motion is transverse to the line of the body, it occasions little comparative inconvenience, because the tendency to propel the blood towards the head is then inconsiderable.

The regularity of the motion in swinging, afforded me an apparently favorable opportunity for trying the effect of inspiration; but although the advantage was manifest, I must confess, it did not fully equal the expectations I had formed from my experience at sea. It is possible that the suddenness of the descent, may in this case be too great to be fully counteracted by such means; but I am inclined to think that the contents of the intestines, are also affected by the same cause as the blood; and if these have any direct disposition to regurgitate, this consequence will be in no degree counteracted by the process of respiration.

A friend of mine informed me that he had endeavoured to counteract this mechanical effect upon the stomach, and had experienced immediate relief from a slight degree of sea-sickness, by lying down upon the deck with his head towards the stem of the vessel; by means of which, upon pitching, he was in the attitude of a person descending backwards in a swing.

Whether the stomach be or be not thus primarily affected, or only by sympathy with the brain, the sensation of sinking is in all cases referred directly to the stomach, which is seized with such instantaneous retching, that no person,

who has not been so situated, can form a just conception of it.*

In thus referring the sensations of sea-sickness in so great a degree, to the agency of mere mechanical pressure, I feel confirmed by considering the consequence of an opposite motion, which by too quickly withdrawing blood from the head, occasions a tendency to faint, or that approach to fainting, which amounts to a momentary giddiness with diminution of muscular power. At a time when I was much fatigued by exercise, I had occasion to run to some distance, and seat myself under a low wall for shelter from a very heavy shower. In rising suddenly from this position I was attacked with such a degree of giddiness, that I involuntarily dropped into my former posture, and was instantaneously relieved by return of blood to the head, from every sensation of uneasiness.

Since that time, the same affection has frequently occurred to me in slighter degrees, and I have observed, that it has always been under similar circumstances of rising suddenly

* There is one occasion upon which a slighter sensation of this kind is perceived, and it appears to indicate the direction of the motion from which it arises, to be downwards. "In a country subject to frequent returns of earthquakes" it is said "that "a few minutes before any shock came, many people could foretel it by an alteration in their stomachs; an effect which" (it is added) "always accompanies the wave-like motion of earthquakes, when it is so weak as to be uncertainly distinguishable." (Michell, Phil. Trans. Vol. LI. 610.)

It seems that the vapours to which these tremendous concussions are owing, immense in quantity, and of prodigious force, being for a time confined on all sides, elevate the surface of a country to a vast extent until they either find vent, or meet with some partial cause of condensation, and hence the alternate heaving and subsidence of the ground, will produce much the same effects as the rising and falling of the swell at sea.

* Phil. Trans. Vol. XLII. p. 48.

from an inclined position, after some degree of previous fatigue. Sinking down again immediately removes the giddiness; and then, by rising a second time more gradually, the same sensation is avoided.

Part III. *On the salutary Effects of Riding, and other Modes of Gestation.*

IN the preceding instances of disturbing the circulation of the blood, by external motion, the effect is disagreeable, and proportionally prejudicial. There may indeed be cases of disorder, in which it will be salutary, but these are probably less frequent than is generally supposed.

In the observations which follow, general opinion will concur with me, on the benefit derived from external, or passive motion, and I hope, that in ascribing its good effects to their true cause, I shall enable others to make a valuable distinction, which has not yet been preserved with due care, between one motion which is salutary, and another which is very frequently pernicious. For, although the term *gestation* is employed by medical writers, as a general term comprehending riding on horseback, or in a carriage, and although the merits of such motions, especially the former, were clearly noticed, and perhaps even over-rated, by the discernment of SYDENHAM, I believe that no explanation has yet been given, of the peculiar advantages of external motion, and am persuaded, that the benefits to be derived from carriage exercise, are by no means in so high estimation as they ought to be.

Under the common term *exercise*, active exertion has too frequently been confounded with passive gestation, and

fatiguing efforts have consequently been substituted for motions that are agreeable, and even directly invigorating, when duly adapted to the strength of the invalid, and the peculiar nature of his indisposition.

The explanation which I am about to offer of the effects of external motion upon the circulation of the blood, is founded upon a part of the structure observable in the venous system, the mechanical tendency of which cannot be doubted. The valves which are every where dispersed through those vessels, allow free passage to the blood, when propelled forward, by any motion that assists its progress; but they oppose an immediate obstacle to such as have a contrary tendency. The circulation is consequently helped forward by every degree of gentle agitation. The heart is supported in any laborious effort that may have become necessary, by some obstacle to its exertions; it is assisted in the great work of restoring a system, which has recently struggled with some violent attack; or it is allowed, as it were, to rest from a labour, to which it is unequal, when the powers of life are nearly exhausted by any lingering disorder.

In the relief thus afforded to an organ so essential to life, all other vital functions must necessarily participate; and the various offices of secretion, and assimilation, by whatever means they are performed, will not fail to be promoted during such comparative repose from laborious exertion.

Even the powers of the mind itself, though apparently, least likely to be influenced, by mere mechanical means, are manifestly, and in many persons, most immediately affected, by these kinds of motion.

It is not only in cases of absolute deficiency of power to

carry on the customary circulation, that the beneficial effects of gestation are felt; but equally so, when comparative inability, arises from redundancy of matter to be propelled. When from fulness of blood, the circulation is obstructed, the whole system labours under a feeling of hurry and agitation, with that sensibility to sudden impressions, which is usually termed nervousness. The mind becomes incapable of any deliberate consideration, and is impressed with horrors that have no foundation, but in a distempered imagination.

It is in moderate degrees of this species of affection, that the advantages of carriage exercise, are most sensibly felt. The composed serenity of mind that succeeds to the previous alarm, is described by some persons, with a degree of satisfaction, that evinces the decided influence of the remedy. With this steadier tone of mind, returns its full power of cool reflection; and if the imagination becomes more alive than usual, its activity is now employed in conceiving scenes that are amusing, and agreeable.

As an instance of direct relief to a circulation, labouring from mere fulness of blood, I may adduce that of a person, whose friends, as well as himself, were apprehensive, from the violent and visible throbbing of his heart, of the existence of some organic mischief, and were in some measure alarmed for the consequences.

He was persuaded, and not reluctantly, to go without delay, for medical advice, and was accordingly conveyed in a carriage to the house of some physician of eminence, but did not succeed in finding him at home. As the symptoms did not appear to admit of delay, and were at least not aggravated by the motion, it was hoped that the wished-for advice might be

obtained at a part of the town, which happened to be at some distance. But the second attempt proved as fruitless as the former, and a third was made with the same event. Since the throbbing had by that time considerably abated, he was contented to postpone any further efforts to the following day, and directed the carriage homewards. By the time that he returned to his friends, he found, that the motion of travelling over several miles of pavement, had apparently removed the complaint. The pulsation of the heart and arteries had subsided to their natural standard, and he congratulated himself, that his search of a remedy had not been ineffectual, although he had been disappointed as to the source from which he thought he had most reason to expect relief.

If vigour can in any instance be directly given, a man may certainly be said to receive it in the most direct mode, when the important service of impelling forward the circulation of his blood is performed for him by external means. The main spring, or first mover of the system, is thereby, as it were, wound up; and although the several subordinate operations, of so complicated a machine, cannot be regulated in detail, by mere external agency, they must each be performed with greater freedom, in consequence of this general supply of power.

In almost every treatise on the subject of chronical diseases, are to be found numerous instances of the benefit, produced by the several modes of gestation, which have been most generally adopted; as riding on horseback, in carriages, sea-voyages, and swinging. And in many cases, which might be adduced, it has appeared too clear, to admit of a doubt, that the cure of the patient, has been owing *solely* to the external

agitation of his body, which must be allowed, at least, to have had the effect above explained: that of relieving the heart and arteries, from a great part of their exertion in propelling the blood, and *may* therefore have contributed to the cure, by that means only.

The different modes above mentioned, are adapted from their nature to different degrees of bodily strength, and if there are cases in which, that which appears most eligible, may not suit the situation, or circumstances of the patient, it can not be difficult, to contrive other means of giving motion, so as least to incommode, and yet to give the greatest relief. A very gentle and long continued, or even incessant motion, may suit some cases better than any more violent and occasional agitation; and in this way, probably, it is, that sea voyages have sometimes been attended with remarkable advantage.

II. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches, on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogene.* By Humphry Davy, *Esq. Sec. R.S. F.R.S.E. M.R.I.A.*

Read November 16, 1809.

1. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental enquiries, on the application of Electricity to Chemistry, the commencement and progress of which, this learned Body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

II. *Some new Experiments on the Metals from the fixed Alkalies.*

IN the paper, in which I first made known potassium and sodium to the Royal Society, I ventured to consider these

bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phænomena.

Since that time, various repetitions of the most obvious of the experiments on this subject, have been made in different parts of Europe. The generality of enlightened chemists, have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of enquiry are new, and removed from the common order of facts, some enquirers have given hypothetical explanations of the phænomena, different from those I adopted.

MM. GAY LUSSAC and THENARD, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda, with hydrogene; a similar opinion seems to be entertained by M. RITTER. M. CURADAU* affects to consider them as combinations of charcoal, or of charcoal and hydrogene, with the alkalies; and an Enquirer † in our own country, regards them as composed of *oxygene* and *hydrogene*.

I shall examine such of those notions only, as have been connected with experiments, and I shall not occupy the time of the Society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which MM. GAY LUSSAC and THENARD, derive their inferences. At the time that these papers were

* Journal de Physique June, 1808. † Nicholson's Journal. August, 1809. p. 258.

written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur, and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their enquiry in the second volume of the Mem. d'Arcueil, a copy of which M. BERTHOLLET has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the Society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium* in ammonia, and they found that a considerable quantity of ammonia was absorbed; and hydrogene produced; and that the potassium became converted into an olive coloured fusible substance; by heating this substance strongly, they obtained three fifths of the ammonia again, two fifths as ammonia, one fifth as hydrogene and nitrogene; by adding a little water to the residuum, they procured the remaining two fifths, and found in the vessel in which the operation was carried on, nothing but potash.—Again, it is stated, that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogene, and an absorption of the ammonia; and by carrying on the operation, they affirm, that they can procure from a given quantity of ammonia, more than its volume of hydrogene.

Whence, they ask, can the hydrogene proceed?—shall it be

* Mem. d'Arcueil, Tom. II. page 309.

admitted that it is from the ammonia? but this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. BERTHOLLET, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogene gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogene, and that alkali.

It is obvious, that even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygene which this would give to the potassium, and of the hydrogene which it might furnish, to reproduce the ammonia, they take no notice.

I have shewn, by numerous experiments, many of which have been repeated before members of this Society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion, is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hydrogene and nitrogene, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogene.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, shew this; and they likewise shew that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions, and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from 3 to 4 grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases, there was a considerable deficiency of hydrogene, as well as nitrogene, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases, I obtained more than four fifths of the potassium which had been employed; and very nearly the whole of the nitrogene, existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at 30.2$^{in.}$, thermometer at 54° FAHRENHEIT. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7.5 were ab-

sorbed, and 3.2 of hydrogene evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors was exhausted, and filled with hydrogene. They contained together $\frac{1}{10}$ of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{4}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7.5 of oxygene, and acted upon by the electrical spark, left a residuum of 7.5. The quantity of potassium formed, was such as to generate by its action upon water, three cubical inches, and $\frac{1}{10}$ of hydrogene gas.

Now if this experiment be calculated upon, it will be found, that 7.5 — .2 = to 7.3 of ammonia, by its electrical decomposition, would afford about 13.1 of permanent gas, containing 3.4 of nitrogene, and 9.7 of hydrogene. But the 3.2 cubical inches of hydrogene, evolved in the first part of the process, added to the 5.8 evolved in the second part of the process = 9; and the nitrogene in the 8.8 cubical inches of gas, (or the 9—.2 of ammonia,) will be about 3, and if we estimate .84 of hydrogene, and .16 of nitrogene, in the .5, remaining in the retort; there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the $\frac{1}{10}$ of hydrogene pre-existing in the tube and adaptors, the loss of hydrogene will be found proportionally rather greater than that of nitrogene.

In another experiment in which 3 grains of potassium were employed in the same manner, 6.78 cubical inches of ammonia

were found to be absorbed, and 2.48 of hydrogene only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors, the same quantity of residual air, as in the process last described.

The 8 cubical inches of gas contained scarcely $\frac{1}{7}$ of a cubical inch of ammonia; and the unabsorbable part detonated with oxygene, in the proportion of 11 to 6, gave a residuum of 7.5. — The barometer was at 30.2^{in}, thermometer at 52° FAHRENHEIT. Dr. PEARSON, Mr. ALLEN, and Mr. PEPYS were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6.78 — .4 of ammonia = 6.38, and this quantity of gas decomposed by electricity, would afford 11.4 of permanent gas, consisting of 2.9 nitrogene, and 8.5 hydrogene; but there are produced in this experiment, of hydrogene, 2.48 in the first operation, and 4.28 in the second, and considering the nitrogene in the permanent gas as 3.82, .8 must be subtracted for the common air; which would give 2.52 for the nitrogene generated; and to these must be added, the quantity of hydrogene and nitrogene in the tubes and adaptors.

The quantity of potassium regenerated, was sufficient to produce 2.9 cubical inches of hydrogene.

In all experiments of this kind, a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature

below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort, containing oxygene gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from 3 to 4 grains of potassium, were made to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from 4 to 6 grains of this powder formed; but I have advanced no further in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxygene.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture, and the appendix, there is always a loss of nitrogene, a conversion of a portion of potassium into potash, and a production of hydrogene. When copper tubes are employed, the hydrogene bears a smaller proportion to the nitrogene; and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium or nitrogene; but a loss smaller, or greater of hydrogene.

It will be asked, on what do these circumstances depend? Do the affinity of certain metals for potassium, prevent it from gaining oxygene from ammonia, and do platina and copper

combine with a small quantity of hydrogene, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogene has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the enquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded, but the volatile alkali, and that the *hydrogene* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygene and hydrogene.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

3.$\frac{1}{10}$ grains of sodium I found absorbed 9.1 of ammonia, and produced about 4.5 of hydrogene, and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and by detonation with oxygene, proved to consist of nearly two of hydrogene, to one of nitrogene. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phænomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal which is decompounded in this process.

As sodium does not act so violently upon oxygene, as potassium; and as soda does not absorb water from the atmosphere, with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form evidently, by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogene.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies, are composed of the *alkalies* merely united to *charcoal*; the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which MM. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxydated, carbonic acid is formed. This I have never found to be the

case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in 8 cubical inches of oxygene: nearly two cubical inches of oxygene were absorbed, and soda in a state of extreme dryness, so that it could not be liquified by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phænomena which I have described in the Bakerian lecture for 1807. Nearly 6 cubical inches of hydrogene were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination, and it appears from M. CURAUDAU's method of decomposing the alkalies, that his metals must have been carburets not of potash and soda, but of potassium and sodium.

M. RITTER's argument in favour of potassium and sodium, being compounds of hydrogene, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygene than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogene; yet though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,* yet sodium is heavier than potassium in the proportion of 9 to 7 at least.

* Hassenfratz, Annal. de Chem. Tom. XXVIII. page 11.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygene than sodium; and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxygene is given off at the positive surface, no hydrogene at the negative surface, but a brown powder which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogene, than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical enquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find that instead of proving that potassium is a compound of potash and hydrogene, they confirm the idea of its being as yet like other metals undecompounded.

When tellurium is made the positive surface in water, oxygene is given off, when it is made the negative surface, the Voltaic power, being from a battery composed of a number of plates exceeding 300; a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown

powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogene in water; which in being diffused, is acted upon by the oxygene of the common air, dissolved in the water, and gradually loses a part of its hydrogene, and becomes a solid hydruret of tellurium. The compound of hydrogene and tellurium produced at the negative pole when uncombined is gaseous at common temperatures, and when muriatic acid, or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large VOLTAIC apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which when touched by water, did not inflame nor effervesce, but rendered the water of a beautiful purple colour, and when thrown into water entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogene, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *telluretted* hydrogene, (if the name may be used,) had formed with the oxidated potassium, i. e. the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogene; metallic tellurium was formed where

it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was further confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogene gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogene was evolved by the action of the alloy upon water; but the compound of telluretted hydrogene and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition of potash*, might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 50 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal,

which Mr. HATCHETT had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aeriform* compound of tellurium and hydrogene; I shall mention merely some of its most remarkable qualities, and agencies, which, as will be shewn towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hydrogene, is more analogous to sulphuretted hydrogene, than to any other body. The smell of the two substances is almost precisely the same.* Its aqueous solution is of a claret colour;

* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend JOHN GEORGE CHILDREN, Esq. of Tunbridge, in which Mr. CHILDREN, Mr. PEPYS, and Mr. WARBURTON co-operated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of VOLTAIC electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets, at the VOLTAIC negative surface, rather confirmed the suspicion, for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogene in the voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of baryte, induced me to resist the inference; and further researches, made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic; but which is soon converted into muriate of tellurium.*

As arsenic has an affinity for hydrogene, it occurred to me as probable, that it would present some phænomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in

* From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogene, enters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogene by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic* were heated together in hydrogene gas, they combined with such violence as to produce the phænomena of inflammation, and an alloy was produced of the same kind as that formed by means of the VOLTAIC battery.

As tellurium and arsenic both combine with hydrogene, it appeared to me probable, that by the action of alloys of potassium, with tellurium and arsenic, upon ammonia, some new phænomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendour, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogene, instead of being pure hydrogene, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon

* In reasoning upon the curious experiment of CADET, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy Consult. Chem. Tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that though potash is decompounded in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogene. The gasses not absorbable by water given off in this operation, are peculiar. Their smell is intensely foetid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogene: whether they are mixtures of various gasses, or a single compound, I am not at present able to decide.

ammonia, likewise produced a gas which was principally nitrogene, so that if it be said that the metal, and not the volatile alkali is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogene, and in others a compound of hydrogene, which are contradictory assumptions.

None of the chemists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agents, which they did not suspect were concerned. No person, I believe, has attempted to shew that when potassium or sodium is burnt in oxygene gas, water is formed, or that water is generated, when potassium decomposes any of the acids,* and no one has been able to form potassium, by combining hydrogene with potash. I stated in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxygene

* When in October 1807, I obtained a dark coloured combustible substance from boracic acid, at the negative pole in the VOLTAIC circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. MM. GAY LUSSAC and THENARD have likewise operated upon boracic acid, by potassium, and they conclude that they have decompounded it; but this does not follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash; the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

gas, *the pure* alkalies were formed in a state of extreme dryness; and that 100 parts of potassium, absorb about 18 parts of oxygene, and 100 parts of soda about 34 parts. Though in the experiments from which these deductions were made, very small quantities only of the materials were employed, yet still from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made, upon considerable portions of potassium and sodium, procured by chemical decomposition.

When potassium is burnt in trays of platina, in oxygene gas that has been dried by ignited potash, the absorption of oxygene is about $\frac{11}{20}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.* The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'ARCET has shewn by some very well conducted enquiries, that potash and soda,† in their common state, contain a considerable proportion of water; and M. BERTHOLLET concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 19.89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been

* The quantities of gas given out by the operation of water, are in a similar ratio. See page 43 of the last Baker an lecture, and page 26 of this paper.

† Annales de Chimie. Nov. 1808, page 175.

ignited, contains in the 100 parts 66.66 potash, and 33.34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogene holding potassium in solution, was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from 10 parts of potassium, 17.5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner: 5 grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light;* and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about 5 of hydrogene were produced. The increase of weight of the tray was about 4.5 grains; and it did not lose any weight by being ignited.

* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

The second experiment was conducted with still more attention to minuteness. 8 grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phænomena as in the last experiment, and the increase of weight of the tray was $6\frac{1}{2}$ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1.4 grains of oxygene, to form 9.4 grains of potash, and $6.6 - 1.4 = 5.2$, the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35.6 of acid, and 64.4 of potash; but 35.6 of muriatic acid according to M. BERTHOLLET's estimation, would demand 71.1 of alkali, in the state of dryness in which it exists in muriate of potash, and $71.1 - 64.4 = 6.7$ — so that the potash taken as a standard by M. BERTHOLLET, contains at least 9 per cent. more water, than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali.*

* Consequently M. BERTHOLLET's fused potash must contain nearly 23 per cent.

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium, can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogene in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides; this part of the enquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

III. *Experiments on Nitrogene, Ammonia, and the Amalgam from Ammonia.*

One of the queries that I advanced, in attempting to reason upon the singular phænomena produced by the action of potassium upon ammonia was, that nitrogene might possibly consist of oxygene and hydrogene, or that it might be composed from water.

I shall have to detail in this section, a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have

of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat, contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium, as the dry standard.

been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same paths of enquiry, and which at first view do not appear unpromising.

The formation of nitrogene has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists, on the origin of nitrogene, produced during the passage of water through red hot tubes, or the speculations of GIRTANNER, founded on these and other erroneous data; the early discovery of PRIESTLEY on the passage of gasses through red hot tubes of earthen ware, the accurate researches of BERTHOLLET, and the experiments of BOUILLON LA-GRANGE, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogene has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity.* To ascertain if nitrogene could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by VOLTAIC electricity, so as to produce oxygene and hydrogene with great rapidity, and in which these gasses could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the VOLTAIC, and common electrical circuit, were

* See Dr. PEARSON's elaborate experiments, on the decomposition of water by electrical explosions. NICHOLSON's Journal, 4to. vol. I. page 301.

hermetically inserted into the tube. 500 double plates of the VOLTAIC combination were used, in such activity that about the eighth of a cubical inch of the mixed gasses, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubic inch; it had been carefully purged of air by the air-pump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygene and hydrogene, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gasses; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water i. e. $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygene; 6 measures mixed with 8 measures of oxygene diminished to 5; so that it consisted of 2.6 of hydrogene, and 3.4 of a gas, having the characters of nitrogene.

This experiment seemed in favour of the idea of the production of nitrogene from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires under oil, and carrying on the process as before. In this experiment the residuum did not

seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined: six measures of it detonated with 3 measures of oxygene, diminished to rather less than 1 measure. A result which seems to shew, that nitrogene is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydrogene. That the hydrogene is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. CAVENDISH on the deflagration of mixtures of oxygene, hydrogene, and nitrogene, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogene, mixed with the oxygene and hydrogene, and is never produced from those two gasses alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to shew that the nitrous acid, which appears in many processes of the VOLTAIC electrization of water, cannot be formed, unless *nitrogene* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated.* The energy with which the large VOLTAIC apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test, than was before in my power. I had formerly found in an experiment, in which pure water

* NICHOLSON's Journal, August, 1809, p. 258.

was electrified in two gold cones in hydrogene gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogene dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygene gas, and the other in an apparatus, in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{13}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gasses were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases, for from two to three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxygene, from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogene was present, the indications of the production of acid and alkaline matter, were very feeble; though if the asbestus was touched with unwashed hands, or the smallest particle of neutro saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygene and hydrogene produced from the water, were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a

combination of 500 plates, and continued for some days, it was always found that fixed alkali, separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry, is not decomposed by silex, it seems very likely that muriatic acid in its arid state, may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by VOLTAIC electricity, in mixtures of the vapour of water and oxygene gas. I thought it possible if water could be combined with *more oxygene*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygene was mixed with nitrogene, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made red hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid was however generated; but upon this substance a single trial only was made, and that in a small

tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited by the action of water, in the manner stated by Dr. WOODHOUSE, I failed in the trial in which the mixture was cooled in contact with hydrogene. I have since made a number of similar experiments. In general when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phænomena of the process could be more rigourously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner, that the mixture could be cooled in contact with the gas, produced during the operation; and that water exhausted of air, could be made to act upon the cooled mixture, and afterwards, distilled from it; figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carrried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogene from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was

found to hold in solution, small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. BERTHOLLET, that nitrogene adheres very strongly to charcoal.* The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogene is present, are perhaps against the idea that nitrogene is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. PRIESTLEY upon the production of nitrogene, during the freezing of water, induced that philo-

* Mem. d'Arcueil, Tom. II. page 485.

sopher to conceive, either that water was capable of being converted into nitrogene, or that it contained much more nitrogene than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing, there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{50}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{100}$ oxygene, $\frac{3}{100}$ hydrogene, and $\frac{6}{100}$ nitrogene.

Mr. KIRWAN observed the fact that when nitrous gas, and sulphuretted hydrogene, are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results, and I found, that the diminution of the volume of the gasses when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

In reasoning upon this phænomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogene, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of

this lecture, contains a volume of hydrogene, equal to its own. But one of hydrogene demands half its volume of oxygene to convert it into water, and nitrous gas consists of about half a part in volume of oxygene; so that supposing the whole of the hydrogene employed in absorbing oxygene from nitrous gas, nitrogene alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogene of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogene over mercury, the barometer being at 29.5$^{in.}$, thermometer at 51° FAHRENHEIT; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gasses quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at 30.45$^{in.}$, and thermometer 52° FAHRENHEIT; when it equalled 2.3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as BOYLE's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogene, and these complicated changes, ended in the production of two new compounds; nitrogene, hydrogene; oxygene and sulphur combining to form one; and a part of the nitrogene and oxygene, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompound nitrogene, by agents which I conceived might act at the same time on oxygene, and on the basis of nitrogene. Potassium, as I have before stated, sublimes in nitrogene, without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogene, assisted by the intense heat and decomposing energy of VOLTAIC electricity.

I had an apparatus made, by which the VOLTAIC circuit could be completed in nitrogene gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina, it could be fused and sublimed in the gas. The quantity of nitrogene, was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phænomena were very brilliant; as soon as the contact with the potassium was made there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygene gas.

In all trials of this kind, hydrogene was produced; and in some of them there was a loss of nitrogene. This at first seemed to lead to the inference that nitrogene is decompounded in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hydrogene in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogene lost.

The largest proportion of nitrogene which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogene, nearly equal to $\frac{1}{4}$ of the nitrogene, was produced. It cannot be said, that the nitrogene is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogene, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced, when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state, is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogene gas; a little phosphuretted hydrogene was given off from it, but the nitrogene was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogene, are readily decomposed by oxymuriatic acid gas, a mixture was

of nitrogene and oxymuriatic acid gas, was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogene; but the greatest part came over densely clouded, and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogene can be drawn from the process.

The general tenour of these enquiries cannot be considered as strengthening in any considerable degree, the suspicion which I formed of the decomposition of nitrogene, by the distillation of the olive coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phænomena in this operation, it appears to me indeed possible to account for the loss of nitrogene, without assuming that it has been converted into new matter. Though the iron tubes which I used, were carefully cleaned; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxygene of which, in the beginning of the process of distillation, might form water with hydrogene, given off from the fusible substance; which being condensed in the upper part of the tube; would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogene, and the production of an increased proportion of hydrogene. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydro-

gene produced. Thirteen cubical inches of gas, were evolved in the first operation; which consisted of nearly 1 cubical inch of ammonia, 4 of nitrogene, and 8 of hydrogene. The portion of gas given off in the second operation, equalled 8.6 cubical inches; which consisted of 2.5 hydrogene, and 1.1 nitrogene. The potassium produced in the operation, was sufficient to generate 3.1 cubical inches of hydrogene.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. HASSENFRATZ,* that the loss of so large a portion of potassium, might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2.3 cubical inches of hydrogene were found to be generated.

In the experiments detailed in page 58 of the last volume of the Transactions, a loss of nitrogene, and a production of hydrogene was perceived in a case in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naptha*, and it is possible that ammonia might have been regenerated by hydrogene from the naptha, and absorbed by that fluid; and a part of the hydrogene might likewise proceed from the decompo-

* Journal des Mines. Avril 1808. p. 275.

sition of the naphtha; and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogene.

Even the considerable excess of hydrogene, and deficiency of nitrogene, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against the mode of explaining the phænomena by supposing nitrogene decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society, a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygene was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phænomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygene in this substance, at the same time it led to the suspicion, that of the two gasses separated by electricity, one, or perhaps both, might

contain metallic matter united to oxygene: and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by VOLTAIC and common electricity; and I have used an apparatus (of which a figure is attached to this paper,) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before, and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gasses,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogene and nitrogene, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive coloured matter formed on the metal, but though in this case nearly 8 cubical

inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to —15°, there was a very feeble indication given of the addition of hygrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts,) from 100 to 185, and the hydrogene was to the nitrogene, in the average proportions of from 73.74 to 27.26; the proper corrections being made, and the precautions before referred to, being taken.*

Assuming the common estimations of the specific gravity of ammonia, of hydrogene, and nitrogene, the conclusions which I have advanced in the Bakerian lecture for 1807, would be supported by these new experiments, but as the

* Philosophical Transactions 1809, page 459. M. Berthollet, Jun. in the second volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxygene, separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and at the same time he confutes some experiments which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points, appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygene were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia, seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to over-rate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogene may be to the hydrogene as one to three; but, neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferrences that may be made, but it cannot be regarded as an absolute fact.

moisture and oxygene visibly separated, cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia; I resolved to investigate more precisely, than I had reason to think had been hitherto done, the specific gravities of the gasses concerned in their dry state; and the very delicate balance belonging to the Royal Institution, placed the means of doing this in my power.

Nitrogene, hydrogene, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at 30.5$^{in.}$ barometer, 51° FAHRENHEIT's thermometer.

For nitrogene, the 100 cubical inches - - 29.8 grains.
For hydrogene, ditto. - - - - 2.27
For ammonia - - - - - - 18.4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gasses evolved; there is a loss of $\frac{1}{13}$,* and if the smallest proportion be taken the loss will be nearly $\frac{1}{12}$.

These results and calculations agree with those that I have before given, and with those of Dr. HENRY.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

* 100 of ammonia at the rate of 185, will give 136.9 of hydrogene, weighing 3.1 grains, and 48.1 of nitrogene weighing 14.33 grains; but $18.4 - 17.4 = 1$, and at the rate of 180, 133 of hydrogene weighing 3.01 and 47 of nitrogene, weighing 14 and $18.4 - 17 = 1.4$.

It is *possible*, that the minute quantity of oxygene which appears to be separated is not accidental, but a result of the decomposition; and if hydrogene and nitrogene be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogene and nitrogene alone, by electricity, and that the loss of weight, is no more than is to be expected in processes of so delicate a kind, is in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogene and hydrogene, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions, intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct, than when pure mercury was used.

By exposing the mercury to a cold of —20° FAHRENHEIT, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogene gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hydrogene produced.

As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogene and ammonia produced, if no more water was present, than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are

taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide,) must contain about 48 per cent. of oxygene, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.*

If hydrogene be supposed to be a simple body, and nitrogene an oxide, then on the hypothesis above stated, nitrogene would consist of nearly 48 of oxygene, and 34 of basis; but if the opinion be adopted, that hydrogene and nitrogene are both oxides of the same metal, then the quantity of oxygene in nitrogene, must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phænomena of chemical science, they perhaps might be more easily explained on the notion of nitrogene being a basis, which became alkaline, by combining with one portion of hydrogene, and metallic, by combining with a greater proportion.

* Even in common air, the amalgam evolves hydrogene and ammonia, nearly in these proportions, and in one experiment which I lately tried, there seemed to be no absorption of oxygene from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogene be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rapidly absorb oxygene. In my first experiments upon the amalgam, finding that common air to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygene had been absorbed; but this difference might have arisen partly at least, from the mixture of hydrogene. Whether in any case the amalgam absorbs oxygene gas, is a question for further investigation.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter, as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at $\frac{1}{12000}$, but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56, be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1600}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully these ideas, and I shall conclude this section by stating, that though the researches on the decomposition, and composition of nitrogene, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese, may be applied to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

IV. *On the Metals of Earths.*

I have tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of

the common earths, as those afforded by the electrochemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogene gas; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind, as that which has been employed in the experiment.

I have passed potassium in vapour through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali, was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the

* The results of this experiment, are opposed to the idea that potassium is a compound of hydrogene and potash or its basis, for if so, it might be expected that the hydrogene would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogene as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

water, globules of gas were slowly liberated, and the effect continued for nearly an hour; so that there is great reason to believe, that the silex had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left as the result of its combustion, alkali and silex. The part which did not inflame, was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

Potassium in acting upon alumine and glucine, produced more hydrogene than could be ascribed to the moisture present in the crust of potash; from which it seems probable that even after ignition, water adheres to these earths.

The results of the action of the potassium, were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks,* and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the

* The pyrophorus from alum, which I have supposed in the last Bakerian lecture, to be a compound of potassium, sulphur, and charcoal, probably contains this substance likewise.

potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them, which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths, to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury, whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystaline texture. The alloys from alumine and glucine, were imperfectly malleable. Each afforded by solution in acids, evaporation, and treatment with re-agents oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the pro-

duction of an amalgam, from the metals of the common earths, yet I succeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia, was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogene gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygene which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies, may likewise be ascertained.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action, may be different exhibitions of the same property of matter, and that oxygene and inflammable bodies, are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that the attractions of acids for salifiable bases, would be inversely, as the quantity of oxygene that they contain, and supposing the power of attraction to be measured by the

quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygene and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808, concluded that barytes must contain least oxygene of all the earths, and that the order as to the quantity of inflammable matter, must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygene of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90.5 * of metal per cent. strontites 86,† lime 73.5,* magnesia, 66.‡

The same proportions would follow from an application of Mr. DALTON's ingenious supposition, § that the proportion of

* Mr. JAMES THOMPSON, NICHOLSON's Journal, 1809, p. 175, and BERTHIER.

† Mr. CLAYFIELD. THOMSON's Chemistry, Vol. II. p. 626, 629.

‡ MURRAY's Chemistry, Vol. III. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis, being inversely, as the quantity of oxygene contained by the basis, though gained from the comparison of the electrical relations of the earths, with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. DALTON's law of general proportions. Mr. DALTON had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. GAY LUSSAC's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygene, might (as far as it is correct) be inferred from Mr. DALTON's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. DALTON, there is a proportion of oxygene, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygene and of acid, are always multiples of these proportions. So that if a protoxide in becoming a deutoxide, takes up more acid, it will be at least double the quantity, and in these cases, the oxygene will be strictly as the acid. Mr. DALTON's law even provides for cases to which M. GAY LUSSAC's will not apply, a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron per-

oxygene is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, i. e. that every neutral salt is composed of one particle of metal, one of oxygene, and one of acid.

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to RICHTER's estimation of the composition* of phosphate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

M. BERZELIUS,† in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygene.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxygene, which agrees very nearly with the quantity of hydrogene and ammonia produced from the amalgam.

Though the early chemists considered the earths and the

fectly formed, (as some experiments which I have lately made, seem to shew,) there is probably only a single proportion of acid; and in the super-tartrite of potash there is only a single quantity of oxygene, and a double quantity of acid. Whether Mr. DALTON's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

* THOMSON's Chemistry, Vol. II. p. 581.

† In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of M. GAY LUSSAC, on the combinations of gaseous bodies, Mem. d'Arcueil, Tom. II. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by MM. GAY LUSSAC, and THENARD.

metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston, and though LAVOISIER insisted upon this analogy with his usual sagacity, yet still the alkalies, earths, and oxides, have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls;* and the alkalies and alkaline earths, are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shewn by Dr. HENRY and M. GUYTON,) precipitate the triple prussiate of potash; the power of combination is general, but the compounds formed, are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutrosaline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and earths, with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygene, approach very near in their general characters to zircon, silex, and alumine; and in habits of

* KLAPROTH. Annales de Chimie. Tom. X. p. 177.

amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals?

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound, in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold short iron; whilst, from another spot, it will make hot short; but by a combination of the two in due proportions, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy, may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. BERZELIUS informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

HYDROGENE is the body which combines with the largest proportion of oxygene, and yet it forms with it a neutral compound. This on the hypothesis of electrical energy would shew, that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygene by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phænomena presented by sulphuretted hydrogene, are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shewn, and as is further proved by the researches of MM. GAY LUSSAC and THENARD, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decompounded, unless a new combination is formed; thus it is not changed by charcoal ignited in the gas by VOLTAIC electricity; but it is decompounded by all the metals; and in these cases hydrogene is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygene being found in the new compound. This at first view, might be supposed in favour of the idea, that hydrogene is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitromuriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas,) is known to be in a high state of oxygenation.

That nitrogene is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper,) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogene is a protoxide of ammonium, ammonia a deutoxide, and nitrogene a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases, would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,* and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the

* The electrization of sulphur and phosphorus, goes far to prove that they contain combined hydrogene. From the phænomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygene, though as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process, appeared an evidence in favour of their containing oxygene, till I discovered that similar phænomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hydrogene, and on phosphuretted hydrogene, I find that the phænomena differ very much according to the circumstances of the experiment, and in some instances, I have obtained a larger volume of gas from potassium after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the Society. The idea of the existence of oxygene in sulphur and phosphorus, is however still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium I find when heated in hydrogene, mixed with a small quantity of atmospheric air, absorb both oxygene and hydrogene, and become nonconducting inflammable bodies analogous to resinous and oily substances.

suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygene, or by the peculiar state of their electrical energy.

The whole tenour of the antiphlogistic doctrines, necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which if not so simple, account for the phænomena, with at least equal facility.

If hydrogene, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygene, hydrogene, and unknown bases only; metals and inflammable solids will be compounds of these bases, with hydrogene; the earths, the fixed alkalies, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are, First. The properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hydrogene or oxygene, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hydrogene, whether in its simple form, or in combination with one proportion of oxygene, or a double proportion, from my experiments seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hydruret,

seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second. The facility with which metallic substances are revived, in cases in which hydrogene is present. I placed two platina wires, positively and negatively electrified from 500 double plates of 6 inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed; the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogene, is essential to the production of the metal.

Third. Oxygene and hydrogene are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion, it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygene, and is restored by the addition of hydrogene.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogene evolved in my experiments is not exactly the same, as that produced by their action upon water; yet it is probable that this is caused by the imperfection of the process;* and supposing potassium and sodium to pro-

* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogene evolved; i.e. whenever the metals of the alkalies act upon ammonia, (supposing this body to be composed of 3 hydrogene, and 1 of nitrogene, in volume, 2 of hydrogene and 1 of nitrogene remain in combi-

duce the same quantity of hydrogene from ammonia and water, the circumstance at first view, may be conceived favourable to the notion, that they contain hydrogene, which under common circumstances of combination, will be repellent to matter of the same kind; but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogene, are decomposed by a metal, the quantities of hydrogene ought to be the same, or multiples of each other. Thus in the decomposition of ammonia by potassium and sodium, two of hydrogene, and one of nitrogene remain in combination, and one of hydrogene is given off; and in the action of water on potassium to form potash, the same quantity of hydrogene ought to be expelled. From my analysis* of sulphuretted hydrogene, it

nation, and 1 of hydrogene is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogene, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

* The composition may be deduced from the experiments in the last Bakerian lecture, which shew that it contains a volume of hydrogene equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2.27 of hydrogene, and 32.73 of sulphur. When sulphuretted hydrogene is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogene. When it is decomposed by VOLTAIC sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogene, it was found that when these gasses were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common states; the phosphorus was

would appear, that if potassium in forming a combination with this substance sets free hydrogene, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. PROUST and Mr. HATCHETT, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogene, will liberate the same proportion of hydrogene as during its solution in diluted sulphuric acid; and taking Mr. DALTON's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogene, because in acting upon different combinations containing hydrogene, they produce the evolution of equal proportions of this gas, then it might be proved that almost any kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve

dark coloured, and the arsenic appeared as a brown powder, both were probably hydrurets; this is confirmed likewise by the action of potassium upon arseniuretted, and phosphuretted hydrogene; when the metal is in smaller quantity than is sufficient to decompose the whole of the gasses, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogene contain in equal volumes, more hydrogene than sulphuretted hydrogene, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogene, it would appear, that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

the same quantity of hydrogene, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogene in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygene, which may be supposed to exist in hydrogene, that might be applied to explain the composition of nitrogene from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogene and nitrogene; or that a substance which is metallic, can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present, occupy the time of the Society, by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research, have not been sufficiently examined, to enable to decide upon their nature, and their relations, or to form any general theory concerning them, which is likely to be permanent.

EXPLANATION OF THE FIGURES.

Fig. 1. The apparatus for electrizing potassium in gases. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 41. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing, and recomposing water under oil. CC the wires for communicating the GALVANIC electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a. d. c.* the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A VOLTAIC apparatus, being one of the sets which compose the new VOLTAIC battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of £2000 has been raised by subscription, from members of the Royal Institution. As yet the full combination has not been put into action; but results, from the effects of that part of it which have been [illegible] may be expected, from so [illegible] of electrical power.

Fig. 1.

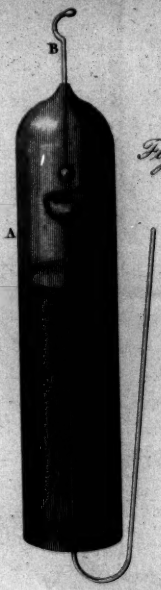


Fig. 2.

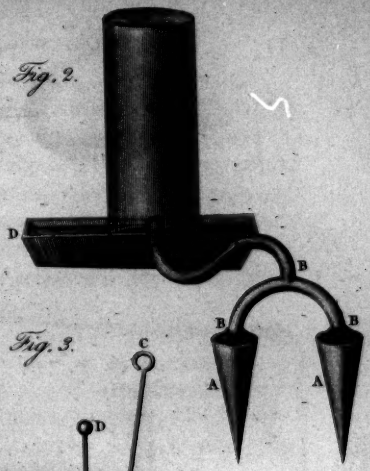


Fig. 3.

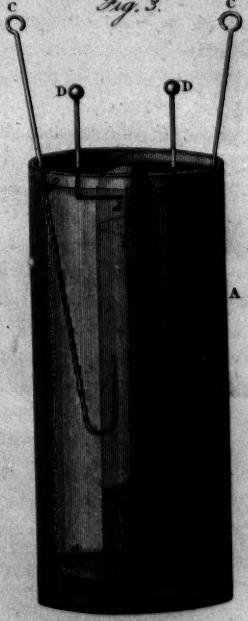


Fig. 5.

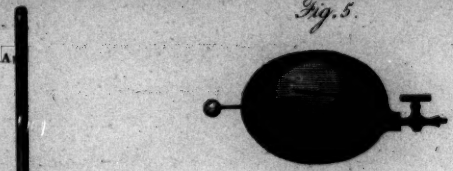


Fig. 4.

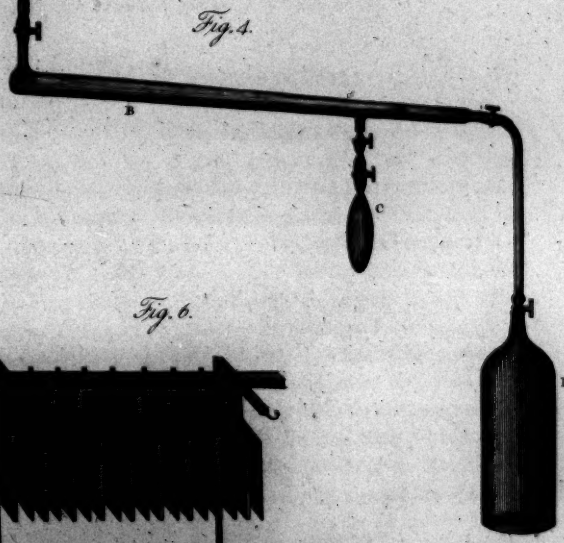
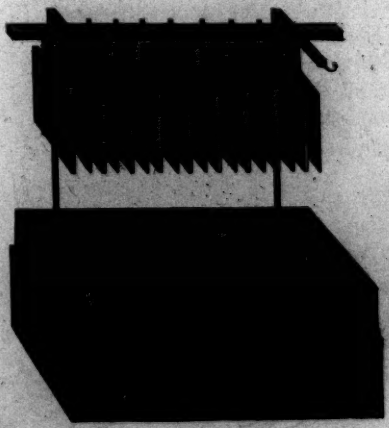
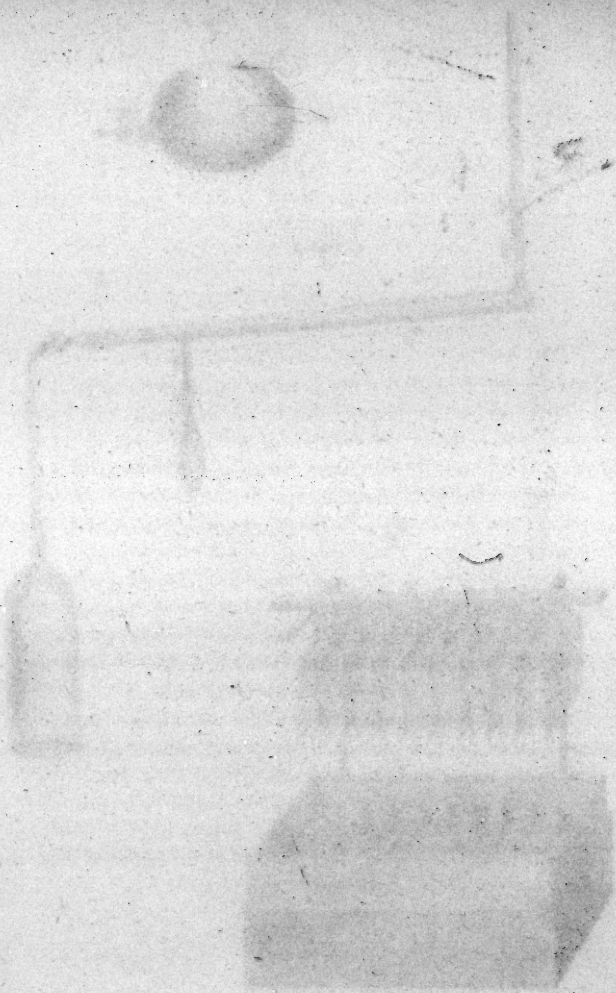


Fig. 6.





III. *The Case of a Man, who died in consequence of the Bite of a Rattle-snake; with an Account of the Effects produced by the Poison.* By Everard Home, *Esq. F.R.S.*

Read December 21, 1809.

Opportunities of tracing the symptoms produced by the bite of poisonous snakes, and ascertaining the local effects on the human body when the bite proves fatal, are of such rare occurrence, that no well described case of this kind, is to be met with in any of the records that I have examined. I am therefore induced to lay before this Society the following account, with the view of elucidating this subject, in which the interests of humanity are so deeply concerned.

Thomas Soper, 26 years of age, of a spare habit, on the 17th of October 1809, went into the room in which two healthy rattle-snakes, brought from America in the preceding summer, were exhibited. He seized one of them with the end of a foot rule, but could not induce the snake to bite it, and on the rule dropping out of his hand, he opened the door of the cage to take it out; the snake immediately darted at the hand, and bit it twice in succession, making two wounds on the back part of the first phalanx of the thumb, and two on the back part of the second joint of the fore finger. The snake is between 4 and 5 feet long, and when much irritated bites the object twice, which I believe snakes do not usually do.

The bites took place at half past two o'clock. He went

immediately to Mr. HANBURY, a chemist in the neighbourhood. There was at that time no swelling on the hand, and the man was so incoherent in his language and behaviour, that Mr. HANBURY considered him to be in a state of intoxication, and gave him a dose of jalap to take off the effects of the liquor, and made some slight application to the bites. It appeared on enquiry, that the man had been drinking, but that before he was bitten, there was nothing unusual in his behaviour. After leaving Mr. HANBURY the hand began to swell, which alarmed him, and he went to St. George's hospital. He arrived there at three o'clock. The wristband of his shirt had been unloosed, and the swelling had extended half way up the forearm before his admission. The skin on the back of his hand was very tense, and the part very painful. At four o'clock the swelling extended to the elbow, and at half past four it had reached half way up the arm, and the pain had extended to the axilla. At this time Mr. BRODIE, who visited him in my absence, first saw him; he found the skin cold; the man's answers were incoherent: his pulse beat 100 strokes in a minute, and he complained of sickness. Forty drops of aqua ammoniæ puræ, and thirty drops of spiritus ætheris vitriolici in an ounce of mistura camphorata, were given to him, but did not remain on his stomach. The wounds were bathed with the aqua ammoniæ puræ, and the arm and forearm had compresses wetted with camphorated spirits applied to them. At five o'clock he took two drams of spiritus ammoniæ compositus, and 30 drops of æther, in an ounce and a half of mistura camphorata, which remained on his stomach. At six o'clock his pulse was stronger; at half past seven his pulse was very feeble, and 30 drops of æther, and the same

quantity of aqua ammoniæ purè were given in water. At half past eight it was repeated. At nine o'clock he had the feeling of great depression, his skin was cold, his pulse weak, beating 80 strokes in a minute. The dose was increased to fifty drops of both medicines, and repeated. At a quarter past ten o'clock the pain had become very violent in the arm: his pulse was stronger, but fits of faintness attack him every fifteen minutes, in which the pulse was not perceptible, but in the interval his spirits were less depressed. In the course of the evening he had two stools. At half past eleven o'clock I first saw him. The hand, wrist, forearm, and arm were much swelled up to the top of the shoulder, and into the axilla. The arm was quite cold, and no pulse could be felt in any part, not even in the axilla, the swelling preventing me from feeling the axillary artery with any degree of accuracy. The wounds made on the thumb were just perceptible; those on the finger were very distinct. His skin generally was unusually cold. I took some pains to diminish his alarm of danger, and found his mind perfectly collected: he said he hoped he should recover. At one o'clock in the morning of the 18th, he talked indistinctly: his pulse beat 100 in a minute; the attacks of faintness came on occasionally. The medicine was repeated every hour.

At eight o'clock in the morning of the 18th, his pulse beat 130 strokes in a minute, and was very feeble. The swelling had not extended beyond the shoulder to the neck, but there was a fulness down the side, and blood was extravasated under the skin as low as the loins, giving the back on the right side a mottled appearance. The whole arm and hand was cold, but painful when pressed; the skin was very tense; on

the inside of the arm below the axilla, and near the elbow, vesications had formed; and under each of the vesications there was a red spot in the cutis, of the size of a crown piece. The skin generally over the body had become warm. He was low and depressed; there was a tremulous motion of his lips, and the faintings recurred at nearly the same intervals as in the preceding evening. The last dose of medicine was rejected by vomiting, but some warm wine remained on his stomach. The arm was fomented. At twelve o'clock, in addition to the above symptoms, there was a starting of his limbs. He had attempted to take some broth, but his stomach did not retain it. The skin of the whole arm had a livid appearance, similar to what is met with in a dead body, when putrefaction has begun to take place, unlike anything which I had ever seen in so large a portion of the living body. An obscure fluctuation was felt under the skin of the outside of the wrist and forearm, which induced me to make a puncture with a lancet, but only a small portion of a serous fluid was discharged. My colleague, Dr. NEVINSON, was present at this visit, and we agreed to continue the internal use of the volatile alkali, with the view of rousing the stomach to action, not considering it as having any specific power over the poison. At eleven o'clock in the evening, finding that his stomach did not always retain the medicines, nor even small quantities of brandy, which were given him, I directed the volatile alkali to be left off, and two grains of opium to be given, and repeated every four hours. At this time his pulse was scarcely perceptible at the wrist, the fainting fits were not less frequent. The vesications and red spots were increased in size.

October 19. At nine o'clock in the morning his pulse was

scarcely perceptible: his extremities were cold; the vesications were larger, and the size of the arm was diminished. He was drowsy, probably from the effect of the opium. He had taken nothing but brandy during the night. At three o'clock in the afternoon he was more depressed: spoke only in whispers: the vesications were increased: the fainting fits less frequent. The arm was diminished in size, and he had sensation in it down to the fingers. At eleven o'clock at night his pulse beat 130 in a minute, and was low. The opium was left off. A stool was procured by clyster. He was ordered to have a glass of camphorated mixture occasionally, and wine and brandy, as often as he could be induced to take them.

October 20. He had dozed at intervals during the night; his spirits were better, and his extremities warmer. At nine o'clock he took coffee for breakfast. He afterwards took some fish for dinner, but it did not remain on his stomach; he therefore took brandy and coffee at intervals, half an ounce at a time, as larger quantities did not remain on his stomach.

October 21. He had slept at intervals during the night, but was occasionally delirious: his pulse 120 in a minute. Brandy and jelly were the only things that stayed on his stomach. The size of the arm was reduced, but the skin was extremely tender.

October 22. He had slept during the greatest part of the night: his pulse beat 98 in a minute: he took some veal for dinner, and brandy at intervals. In the evening his pulse became full and strong: he was ordered wine instead of brandy. The right side of the back down to the loins, was inflamed and painful; and had a very mottled appearance, from the extravasated blood under the skin.

October 23. His pulse continued full, and the arm was very painful, though reduced in size. The vesications had burst, and the exposed cutis was dressed with white ointment. Stools were procured by an opening medicine. He took some veal and porter for dinner; the wine was left off. In the evening he had a saline draught with antimonial wine.

October 24. There was no material change.

October 25. His pulse had increased in frequency, but in other respects he was nearly the same. His bowels were opened by medicine.

October 26. The arm was more swelled and inflamed.

October 27. The inflammation of the arm had increased: his tongue was furred, and his pulse was very frequent. He attempted to sit up, but the weight of the arm, and the pain prevented him. The arm was bathed with spirits of wine and aqua ammoniæ acetatæ in equal quantities.

October 28. A slough had begun to separate from the inside of the arm below the axilla, and a purging had come on, for which he was ordered chalk mixture and laudanum. In the night he had a rigor.

October 29. The purging had abated; his pulse beat 100 in a minute, and was feeble. A large abscess had formed on the outside of the elbow, which was opened, and half a pint of reddish brown matter was discharged with sloughs of cellular membrane floating in it. The lower part of the arm became much smaller, but the upper part continued tense. A poultice was applied to the wound. The lower portion of the arm and the forearm were covered with circular stripes of soap cerate. He was ordered to take the bark, and allowed wine and porter.

October 30. The redness and swelling of the upper part of the arm had subsided: the pulse was 100 in a minute. The purging had returned. The bark was left off: the chalk mixture and laudanum were given, and an opiate clyster administered.

October 31. The pulse beat 120 in a minute. The discharge from the abscess had diminished, the purging continued, and at night he had a rigor.

November 1. The pulse was 120. His voice was feeble; he had no appetite; was delirious at intervals. Ulceration had taken place on the opening of the abscess, so that it was much increased in size. He drank two pints of porter in the course of the day.

November 2. His pulse was very weak; his countenance was depressed; his tongue brown; the ulceration had spread to the extent of two or three inches. Mortification had taken place in the skin nearer the axilla. His stomach rejected every thing but porter: in the night he was delirious.

November 3. The mortification had spread considerably: the purging continued: the forefinger, which had mortified, was removed at the second joint.

November 4. He died at half past four o'clock in the afternoon.

Sixteen hours after death, the body was examined by Mr. Brodie and myself, in the presence of Mr. Maynard, the house surgeon, and several of the pupils of the hospital.

With the exception of the right arm which had been bitten, the body had the natural appearance. The skin was clear and white; and the muscles contracted.

The wounds made by the fangs at the base of the thumb

were healed, but the puncture made by the lancet at the back of the wrist, was still open. That part of the back of the hand, which immediately surrounded the wounds made by the fangs, for the extent of $1\frac{1}{2}$ inch in every direction, as also the whole of the palm, was in a natural state, except that there was a small quantity of extravasated blood in the cellular membrane. The orifice of the abscess was enlarged, so as to form a sore on the outside of the arm, elbow, and forearm, near six inches in length. Around this, the skin was in a state of mortification, more than half way up the outside of the arm, and as far downwards, on the outside of the forearm. The skin still adhered to the biceps flexor muscle in the arm, and flexor muscles in the forearm, by a dark coloured cellular membrane. Every where else in the arm and forearm, from the axilla downwards, the skin was separated from the muscles, and between these parts there was a dark coloured fluid, with an offensive smell, and sloughs of cellular membrane resembling wet tow, floating in it. The muscles had their natural appearance every where, except on the surface, which was next the abscess. Beyond the limits of the abscess, blood was extravasated in the cellular membrane, and this appearance was observable on the right side of the back as far as the loins, and on the right side of the chest over the serratus major anticus muscle.

In the thorax the lungs had their natural appearance. The exterior part of the loose fold of the pericardium, where it is exposed, on elevating the sternum was dry, resembling a dried bladder. The cavity of the pericardium contained half an ounce of serous fluid, which had a frothy appearance, from an admixture of bubbles of air. On cutting into the aorta, a small

quantity of blood escaped, which had a similar appearance. The cavities of the heart contained coagulated blood.

In the abdomen, the cardiac portion of the stomach was moderately distended with fluid: the pyloric portion was much contracted; the internal membrane had its vessels very turgid with blood. The intestines and liver had a healthy appearance. The gall bladder was moderately full of healthy bile. The lacteals and the thoracic duct were empty; they had a natural appearance.

In the cranium the vessels of the pia mater and brain were turgid with blood; the ventricles contained rather more water than is usual, and water was effused into the cells, connecting the pia mater and tunica arachnoides. It is to be observed, that these appearances in the brain and its membranes are very frequently found in cases of acute diseases, which terminate fatally.

The following cases were sent from India, to my late friend Dr. PATRICK RUSSELL: they arrived after his death, and Mr. CLAUDE RUSSELL very kindly gave them to me, knowing the subject of them to be one, in which I had taken an interest. As they correspond in many of the circumstances, with that which has been detailed, I have inserted them in this place, as well as an experiment, which I had an opportunity of making in the West Indies, on the effects of the snake's poison on animals.

A boy, a slave of a gentleman in India, was bitten by a snake called Kamnlee by the natives, in the lower part of the arm, at eight o'clock in the evening. The blood flowed very freely for some time. He died next day at noon in great pain.

A sepoy, 60 years of age, was admitted into the hospital of his regiment, under the care of Mr. PERRIN assistant surgeon, at 4 o'clock in the afternoon of the 15th of October, 1802, in consequence of his being bitten by a Cobra di capello, on the back part of the hand. At the time of his admission he complained of pain running up the arm. He immediately took a drachm of eau de luce, and this dose was repeated every half hour, and the same remedy was applied externally as a lotion to the arm and forearm. At 4 o'clock in the morning of the 16th of October, the pain began to increase, and the arm to swell with great hardness and stiffness, and tumor in the axilla, with much inclination to vomit. He took twelve grains of Dr. JAMES's powder, which brought up a great quantity of bilious matter. He drank copiously of warm water, but no perspiration was induced. He appeared relieved for a short time. At eight o'clock in the morning the arm was distended, painful, and discoloured. He took four ounces of brandy, and repeated it every hour until twelve o'clock, with a drachm of eau de luce occasionally. At this time he was a little revived. The brandy was reduced to two ounces, which were carefully and regularly given every hour, until twelve at noon on the 17th of October, when the arm was more free from pain, but much swelled, hard, and black: his spirits and pulse also were considerably relieved. The eau de luce was now omitted, but the brandy was continued every hour, until twelve o'clock at noon on the 18th of October, when the stiffness and tumor in the axilla had disappeared; the arm was still swelled, but was softer, and less painful. The brandy was omitted: at night he took six grains of Dr. JAMES's powder. On the 19th of October the arm was less, softer, with little or no pain; a

blister was formed and burst on the back of the hand, which discharged three ounces of black fœtid pus. On the 20th, an abscess burst on the hand, in the same situation as the blister, which discharged a large quantity of a fluid having an offensive smell. He was directed to take a drachm of Peruvian bark in port wine, every two hours. On the 22d the swelling was gone, but the discharge was considerable. From this time the man gradually, but slowly recovered, with the loss of the use of his forefinger, which remained permanently extended, and some of the other fingers were affected in a less degree.

In this case, the swelling of the arm was slower in coming on, and less extensive; the pain running up to the axilla, which preceded it, was mistaken for the effect of absorption.

In the year 1782, while in the island of St. Lucia, I made the following experiment.

A spotted dark coloured snake, about two feet in length, having the poison fangs on each side double, with the corresponding surfaces grooved, so as to form a canal for the poison, was put into a square tin box, open at the top, in which a half grown rat was confined. The rat expressed great terror, and remained crouching in one corner of the box, with its eyes fixed on the snake, who lay coiled up at some distance, they were allowed to remain a few minutes in this situation: I then raised one end of the box, which caused the snake to slide along the smooth surface, till it came in contact with the rat, which it immediately bit. The rat died in a minute after the bite. I removed it immediately from the box by means of a pair of long forceps. The wounds made by the fangs were marked by two specks of blood immediately below the shoulder blade. On dividing the skin with a scalpel, the

cellular membrane under it was found entirely destroyed: the muscles were detached from the ribs, and from a small portion of the scapula. The parts immediately surrounding the bite were exceedingly inflamed; as far as I could trust to memory, the appearances very much resembled those produced on the muscles of a dog's thigh, by the application of white arsenic, in consequence of which, death ensued in about sixteen hours.

Fifteen hours after the death of the first, a second rat was bitten by the same snake. This rat was much irritated, and bit the snake in the neck, so violently, that the latter died in about ten minutes. The rat continued very lively for about six hours, and then died. On examination after death, the bite was found to have been inflicted on the left side of the navel, and the abdominal muscles at that part, were in the same state as in the other rat, but in a less degree.

It appears from the facts, which have been stated, that, the effects of the bite of a snake vary according to the intensity of the poison.

When the poison is very active, the local irritation is so sudden and so violent, and its effects on the general system, are so great, that death soon takes place. When the body is afterwards inspected, the only alteration of structure met with, is in the parts close to the bite, where the cellular membrane is completely destroyed, and the neighbouring muscles very considerably inflamed.

When the poison is less intense, the shock to the general system does not prove fatal. It brings on a slight degree of delirium, and the pain in the part bitten is very severe: in about half an hour, swelling takes place from an effusion of

serum in the cellular membrane, which continues to increase with greater or less rapidity for about twelve hours, extending during that period into the neighbourhood of the bite; the blood ceases to flow in the smaller vessels of the swoln parts; the skin over them becomes quite cold, the action of the heart is so weak, that the pulse is scarcely perceptible, and the stomach is so irritable, that nothing is retained in it. In about 60 hours these symptoms go off, inflammation and suppuration take place in the injured parts, and when the abscess formed is very great, it proves fatal. When the bite has been in the finger, that part has immediately mortified. When death has taken place under such circumstances, the absorbent vessels and their glands, have undergone no change similar to the effect of morbid poisons, nor has any part lost its natural appearance, except those immediately connected with the abscess.

In those patients, who recover with difficulty from the bite, the symptoms produced by it, go off more readily, and more completely, than those produced by a morbid poison, which has been received into the system.

The violent effects which the poison produces on the part bitten, and on the general system, and the shortness of their duration, where they do not terminate fatally, has frequently induced the belief, that the recovery depended on the medicines employed; and in the East Indies, eau de luce is considered as a specific, for the cure of the bite of the cobra di capello.

There does not appear to be any foundation for such an opinion; for when the poison is so intense, as to give a sufficient shock to the constitution, death immediately takes

place, and where the poison produces a local injury of sufficient extent, the patient also dies, while all slighter cases recover.

The effect of the poison on the constitution is so immediate, and the irritability of the stomach is so great, that there is no opportunity of exhibiting medicines till it has fairly taken place, and then there is little chance of beneficial effects being produced.

The only rational local treatment to prevent the secondary mischief, is making ligatures above the tumefied part, to compress the cellular membrane, and set bounds to the swelling, which only spreads in the loose parts under the skin; and scarifying freely the parts already swoln, that the effused serum may escape, and the matter be discharged, as soon as it is formed. Ligatures are employed in America, but with a different view, namely, to prevent the poison being absorbed into the system.

IV. *An Analysis of several Varieties of British and Foreign Salt, (Muriate of Soda) with a View to explain their Fitness for different economical Purposes.* By William Henry, *M. D. F. R. S. Vice-Pres. of the Literary and Philosophical Society, and Physician to the Infirmary, at Manchester.*

Read January 25, 1810.

Sect. I. General Observations.

In undertaking the series of experiments, described in the following pages, I had not so much in view the discovery of novelties in science, as the determination, by the careful employment of known processes, and by the improvement of methods of analysis, of a number of facts, the establishment of which (it appeared to me probable) might have an influence on an important branch of national revenue and industry.

An opinion has for some time past existed, and I believe has been pretty general both in this and other countries, to the disadvantage of British salt as a preserver of animal food; and a decided preference has been given to the salt procured from France, Spain, Portugal, and other warm climates, where it is prepared by the spontaneous evaporation of sea water. In conformity with this opinion, large sums of money are annually paid to foreign nations, for the supply of an article, which Great Britain possesses, beyond almost any other

country in Europe, the means of drawing from her own internal resources. It becomes, therefore, of much consequence to ascertain, whether this preference of foreign salt be founded on accurate experience, or be merely a matter of prejudice; and, in the former case, whether any chemical difference can be discovered, that may explain the superiority of the one to the other.

The comparative fitness of these varieties of salt for the curing of provisions, which has been a subject of much controversy among the parties who are interested, can be decided, it is obvious, in no other way, than by a careful examination of the evidence on both sides. Where evidence, however, is doubtful, and where there exists, as in this case, much contrariety of testimony, it cannot be unfair to yield our belief to that, which best accords with the chemical and physical qualities of the substances in question. Again, if salt of British production, should be proved to be really inferior in chemical purity to foreign salt, it would be important to ascertain, as the basis of all attempts towards its improvement, in what, precisely, this inferiority consists. It seemed desirable, also, to examine whether any differences of chemical composition exist among the several varieties of home-made salt, which can explain their variable fitness for economical purposes.

Such were the considerations that induced me to undertake an enquiry, which has occupied, for several months past, a large share of my leisure and attention. I began the investigation, wholly uninfluenced by any preconceived opinions on the subject; and I had no motive to see the facts in any other than their true light, since I have no personal interest, either directly or remotely, in the decision of the question.

The principal sources of the salt, which is manufactured in this country, are rock salt, brine springs, and sea water. The first material is confined entirely, and the second chiefly, though not wholly, to a particular district of Cheshire. Of the extent and boundaries of this district, the process of manufacture, and other circumstances interesting to the mineralogist as well as to the chemist, an ample and excellent history has been given by Mr. HENRY HOLLAND, in the Agricultural Report of the county of Chester.* From his account, I shall extract, in order to render some parts of this memoir more intelligible, a very brief statement of the characteristic differences of the several varieties of salt, which are prepared in Northwich, and its neighbourhood.

In making the *stoved* or *lump salt*, the brine is brought to a boiling heat, which, in brine fully saturated, is 226° of FAHRENHEIT. This temperature is continued during the whole process; and as the evaporation proceeds, small flakey crystals continue to form themselves, and to fall to the bottom of the boiler. At the end of from eight to twelve hours, the greatest part of the water of solution is found to be evaporated; so much only being left, as barely to cover the salt and the bottom of the pan. The salt is then removed into conical wicker baskets, termed *barrows*; and, after being well drained, is dried in stoves, where it sustains a loss of about one seventh of its weight.

On the first application of heat to the brine, a quantity of carbonate of lime, and sometimes a little oxide of iron, both of which had been held in solution by an excess of carbonic acid, are separated; and are either removed by skimming, or are

* Published in 1808.

allowed to subside to the bottom of the pan, along with the salt first formed, and with some sulphate of lime; and are afterwards raked out. These two operations are called *clearing* the pan. Some brines scarcely require them at all, and others only occasionally. The whole of the impurities, however, are not thus removed; for a part, subsiding to the bottom, forms a solid incrustation, termed by the workmen *pan-scale*. The portion of this, which is lowest, acquires so much induration and adhesion to the pan, that it is necessary to remove it, once every three or four weeks, by heavy blows with a pick-axe. These sediments are formed, also, in making the other varieties of salt.

In preparing *common salt*, the brine is first raised to a boiling heat, with the double view of bringing it as quickly as possible to the point of saturation, and of clearing it from its earthy contents. The fires are then slackened, and the evaporation is carried on for 24 hours, with the brine heated to 160° or 170° FAHRENHEIT. The salt, thus formed, is in quadrangular pyramids or hoppers, which are close and hard in their texture. The remainder of the process is similar to that of making stoved salt, except that after being drained, it is carried immediately to the store house, and not afterwards exposed to heat, an operation confined to the stoved salt.

The *large grained flakey salt* is made with an evaporation conducted at the heat of 130 or 140 degrees. The salt thus formed, is somewhat harder than common salt, and approaches more nearly to the cubic shape of the crystals of muriate of soda.

Large grained or *fishery salt*, is prepared from brine heated only to 100° or 110° FAHRENHEIT. No perceptible agitation,

therefore, is produced in the brine, and the slowness of the process, which lasts from 7 or 8 to 10 days, allows the muriate of soda to form in large, and nearly cubical crystals, seldom however quite perfect in their shape.*

For ordinary domestic uses, stoved salt is perfectly sufficient. Common salt is adapted to the *striking* and salting of provisions, which are not intended for sea voyages or warm climates. For the latter purposes, the large grained or fishery salt is peculiarly fitted.

On the eastern and western coasts of Scotland, and especially on the shores of the Firth of Forth, large quantities of salt are made by the evaporation of sea water. In consequence of the cheapness of fuel, the process is carried on, from first to last, by artificial heat, at a temperature, I believe, equal or nearly so to the boiling point, and varying, therefore, according to the concentration of the brine. The kind of salt, chiefly formed in Scotland, approaches most nearly to the character of stoved salt. In some places a salt is prepared, termed *Sunday salt;* so called, in consequence of the fires being slackened between Saturday and Monday, which increases considerably the size of the crystals.

I am indebted to Dr. Thomson of Edinburgh, (who gave me his assistance with great zeal and alacrity) for an opportunity of examining upwards of twenty specimens of Scotch salt, prepared by different manufacturers. That distinguished chemist, it appears from a letter which he addressed to me on the subject, was some time ago engaged in experiments on Cheshire salt. The particulars he has lost; and he retains only a general recollection of the facts, which confirms, I am

* Cheshire Reports, p. 53, &c.

happy to state, the accuracy of the results obtained by my own experiments.

At Lymington in Hampshire, advantage is taken of the greater heat of the climate, to concentrate the sea water by spontaneous evaporation to about one sixth its bulk, before admitting it into the boilers. One kind of salt is chiefly prepared there, which most nearly resembles in grain the stoved salt of Cheshire. The process varies a little, in some respects, from that which has been already described. The salt is not fished (as it is termed) out of the boiler, and drained in baskets; but the water is entirely evaporated, and the whole mass of salt taken out at once, every eight hours, and removed into troughs with holes in the bottom. Through these it drains into pits made under ground, which receive the liquor called *bittern* or *bitter liquor*. Under the troughs, and in a line with the holes, are fixed upright stakes, on which a portion of salt that would otherwise have escaped, crystallizes and forms, in the course of ten or twelve days, on each stake, a mass of sixty or eighty pounds. These lumps are called *salt cats*. They bear the proportion to the common salt, made from the same brine, of 1 ton to 100.

From the mother brine or bitter liquor, which has drained into the pits, the sulphate of magnesia is made during the winter season, when the manufacture of salt is suspended, in consequence of the want of the temperature required, for the spontaneous evaporation of the sea water. The process is a very simple one.* The bitter liquor from the pits is boiled

* I am indebted for an account of this process, as well as of the method of making common salt at Lymington, to the liberal communication of CHARLES ST. BARBE, Esq. of that place. Though not strictly connected with the subject, I give his descrip-

for some hours in the pans, which are used in summer to prepare common salt; and the impurities, which rise to the surface, are removed by skimming. During the evaporation, a portion of common salt separates; and this, as it is too impure for use, is reserved for the purpose of concentrating the brine in summer. The evaporated bitter liquor is then removed into wooden coolers 8 feet long, 5 feet wide, and 1 foot deep. In these it remains twenty four hours, during which time, if the weather prove clear and cold, the sulphate of magnesia, or Epsom salt crystallizes at the bottom of the coolers, in quantity equal to about one eighth of the boiled liquor. The uncrystallizable fluid is then let off through plug-holes at the bottom of the coolers; and the Epsom salt, after being drained in baskets, is deposited in the store-house. This is termed *single* Epsom salts, and after solution and a second crystallization, it acquires the name of *double Epsom* salts. Four or five tons of sulphate of magnesia are produced from a quantity of brine, which has yielded 100 tons of common, and 1 ton of cat salt.

On the banks of the Mersey, near its junction with the Irish Channel, the water of that river before evaporation is brought to the state of a saturated brine, by the addition of rock salt. The advantage of this method of proceeding, will be obvious when it is stated, that 100 tons of this brine yield at least 98 tons of common salt, whereas from the same quantity of sea

tion of the mode of making Epsom salt, because no correct statement of the process has, I believe, been hitherto published. The analysis of sea water, indeed, by a justly distinguished chemist (Bergman), excludes, erroneously, the sulphate of magnesia from its composition; and his results have led to the opinion, that to manufacture this salt on the large scale, requires the addition either of sulphuric acid, or of some sulphate to the *bitter liquor*. (See Aikin's Chemical Dictionary, II. 388.)

water, with an equal expenditure of fuel, only 2 tons 17 cwt of salt can be produced.*

Within the few past years, an attempt has been made to apply rock salt itself to the packing of provisions. For this purpose it is crushed to the proper size between iron rollers. The trials which have been made, I am informed, are but few, and the results hitherto are not perfectly known.

The *bay salt* imported from foreign countries is well known to be prepared by the spontaneous evaporation of sea water, which, for this purpose, is confined in shallow pits, and exposed to the full influence of the sun and air. I have no addition to make to the account of its manufacture, which have already been given by various writers.†

As the results of the investigation, which forms the subject of this memoir, may be acceptable to many persons who can scarcely be expected to take an interest in a long detail of analytical processes, I shall present, in the following section, a general view of the experiments, and of the conclusions that may be deduced from them. In the last place, in order that other chemists may be enabled to repeat the analyses under similar circumstances, I shall describe minutely, the methods that were adopted, some of which are new, and others reduced to greater precision. If, however, in the future progress of science, it should appear that any of these processes are imperfect, it may still be admitted that, for all useful purposes,

* See the Earl of Dundonald's "Thoughts on the Manufacture and Trade of Salt." London, 1785.

† Encyclop. Method. Art. Salins. (Des Marais Salans) Aikin's Dictionary of Chemistry, II. 224. Watson's Chemistry, Vol. II. p. 52. It is necessary to remark, that a great proportion of what is sold in London as bay-salt, is Cheshire large grained fishery salt.

they afford a fair *comparison* of the composition of the several varieties of culinary salt; since the sources of fallacy, that may hereafter be discovered, must have been the same in every case, and have produced in each an error of nearly the same amount.

Section II. General Statement of the Results of the Experiments, and Conclusions that may be deduced from them.

A comparison of the component parts of British and foreign salts, and of different varieties of British salt with each other, will best be made by an examination of the following table, which comprehends the results of the analysis of equal weights of each variety.

	Kind of Salt.	1000 parts by weight consist of: Insol. matter.	Muriate of lime.	Muriate of Magnesia.	Total earthy muriates.	Sulph. of lime.	Sulph. of magnesia.	Total sulphates.	Total impurities.	Pure muriate of soda
Foreign bay salt.	St. Ube's	9	a trace	3	* 3	23 ½	4 ½	28	40	960
	St. Martin's	12	do.	3 ½	* 3 ½	19	6	25	40 ½	959½
	Oleron	10	do.	2	* 2	19 ½	4 ½	23 ½	35 ½	964½
Brit. salt from sea water.	Scotch (common)	4	—	28 or *	28 or *	15	17 ½	32 ½	64 ½	935½
	Scotch (Sunday)	1	—	11 ½	11 ½	12	4 ½	16 ½	29	971
	Lymington (common)	2	—	11	11 or *	15	35	50	63	937
	Do. (cat)	1	—	5	5	1	5	6	12	988
Cheshire salt.	Crushed rock	10	o. 1/16	o. 1/16	o. ½	6 ½	—	6 ½	16 ½	983½
	Fishery	1	o. ½	o. ½	1	11 ½	—	11 ½	13 ½	986½
	Common	1	o. ½	o. ½	1	14 ½	—	14 ½	16 ½	983½
	Stoved	1	o. ½	o. ½	1	15 ½	—	15 ½	17 ½	982½

1. The *total amount of impurities*, and the *quantity of real muriate of soda*, contained in each variety of common salt, may be learned by inspecting the two last columns of the table. From these it appears, that the foreign bay salt is purer,

See page 95.

generally speaking, than salt which is prepared by the rapid evaporation of sea water; but that it is contaminated with about three times the amount of impurities, discoverable in an equal weight of the Cheshire *large-grained* salt, and with more than twice that of those, that are found in the *stoved* and *common* salt of the same district.

II. The *insoluble matter* in the foreign salt, after the action of boiling water, appears to be chiefly argillaceous earth coloured by oxide of iron, and is probably derived in part from the pits in which the sea water is submitted to evaporation. We may, perhaps, assign the same origin to the very minute portion of muriate of lime, which is not found in the salt prepared by evaporating sea water in metallic vessels, nor even in the mother liquor, or uncrystallizable residue. In sea salt prepared by rapid evaporation, the insoluble portion is a mixture of carbonate of lime with carbonate of magnesia, and a fine silicious sand; and in the salt prepared from Cheshire brine, it is almost entirely carbonate of lime. The insoluble part of the less pure pieces of rock salt is chiefly a marly earth, with some sulphate of lime. The quantity of this impurity, as it is stated in the table, is considerably below the average, which in my experiments has varied from 10 to 45 parts in 1000. Some estimate of its general proportion, when ascertained on a larger scale, may be formed from the fact, that government, in levying the duties, allows 65 lb. to the bushel, of rock salt, instead of 56 lb. the usual weight of a bushel of salt.

III. The *earthy muriates*, and especially that with base of magnesia, abound most in salt which is prepared by the rapid evaporation of sea water. Now since common salt, in all its

forms, contains as will afterwards appear, very little water of crystallization, it is probable that the muriate of magnesia, discovered by the analysis of sea salt, is derived entirely from that portion of the mother liquor, which adheres to the salt after being drained, and which amounts to about $\frac{1}{4}$ of its weight. The larger the size of the grain, the less is the quantity of this solution which the salt holds suspended, and hence the salt prepared at a lower degree of heat, being in larger crystals, is less debased by the magnesian muriate, than the salt formed at a boiling temperature. It is probable, also, that when the salt is drawn at intervals from the boiler, the proportion of the earthy muriate will vary with the period of the evaporation, at which it is removed. For it may readily be conceived, that as the proportion of the earthy muriates in any brine is increased by the separation of muriate of soda, the greater will be the quantity of the muriates which the crystals of common salt, formed in the midst of the brine, will retain; thence it follows, that, so far as the earthy muriates only are concerned, salt must diminish in purity as the process of evaporation advances.

In the several varieties of Cheshire salt, the earthy muriates do not exceed one thousandth part of this weight, and they are precisely, (or so nearly so, that the difference is not ascertainable) the same in all. This will cease to be matter of surprise, when it is considered that the salt obtained by evaporating to dryness the whole of a portion of Cheshire brine, does not give more than 5 parts of earthy muriates in 1000. In the entire salt of sea water, according to BERGMAN, the earthy muriates form no less than 218 parts in the same quantity.

According to the proportion in which the earthy muriates are present in any kind of salt, will be its power of deliquescence, or of attracting moisture from the atmosphere. It is not entirely, however, from the salts with earthy base, that common salt derives this quality; for the most transparent specimens of rock salt, which I find to consist of absolutely pure muriate of soda, attract much moisture from a humid atmosphere.

IV. The *sulphate of magnesia* and the *sulphate of lime*, both enter into the composition of all the varieties of salt prepared from sea water; but the sulphate of lime alone is found in Cheshire salt. The proportion of sulphate of magnesia is greatest in that variety of sea salt, which has been formed by rapid evaporation. In foreign bay salt, its quantity is very insignificant.

From the table it may be seen, that the proportion of sulphate of lime is greater in foreign bay salt, than in any variety of British salt, even than in those which are prepared from sea water with a boiling heat. The only explanation of this fact, that occurs to me, is, that during the rapid evaporation of sea water, a considerable part of the calcareous sulphate is precipitated at an early stage of the process, and is partly removed in *clearing* the boiler, a process, which can scarcely be performed during the formation of bay salt, in pits whose sides are composed of moist clay. The remainder of the selenite, thus precipitated by the rapid evaporation of sea water, enters into the composition of the pan-scale.

In the course of this inquiry, I was induced to repeat the same experiments several times, on various specimens of salt bearing the same designation; and was surprised to find, that

the results by no means corresponded. In one instance, for example, fishery salt was found in 1000 parts to contain no less then 16 parts of sulphate of lime; while another specimen, nominally the same, contained only 11¼ parts of selenite in the same quantity; and a third only 5¼. At length it occurred to me, that these differences were probably owing to the circumstance of the salt having been taken from the boiler at different periods of the evaporation. I requested, therefore, to be furnished with specimens of salt, drawn at different stages of the process, from a given portion of brine, evaporated in the same boiler. These were submitted to analysis; and the results are shewn in the following table.

	Contained in 1000 parts. Sulphate of lime.
Common salt drawn from the boiler, 2 hours after the first application of heat. - -	16
Salt drawn 4 hours after do. - - -	11
Salt drawn 6 hours after do. - - - -	8½

Hence it appears, that there was a gradually increasing purity in the salt from sulphate of lime, as the process of evaporation advanced; the greatest part of this earthy compound being deposited at an early stage of the process. Different specimens of the same kind of salt may, therefore, differ in chemical purity as much from each other, as from other varieties. But when the impurities, contained in a solution of muriate of soda, are of a different species, from those of Cheshire brine, and consist chiefly of the earthy muriates, the order will be reversed, and the purest salt, as I have already suggested, will be that which is first deposited; the contamination with the muriate of lime or of magnesia, continuing to increase, as the process advances to a conclusion.*

* I cannot on any other principle explain the considerable differences, as to the

At an early period of the enquiry, it appeared to me probable that the differences between the several varieties of culinary salt might depend, in some degree, on their containing variable proportions of water of crystallization. It was found, however, by experiment, that the proportion of water in any variety of common salt, after being dried at 212° FAHRENHEIT, is not much greater or less than that which is contained in any other variety. Pure transparent rock-salt, calcined for half an hour in a low red heat, (= 4° or 5° of WEDGWOOD'S pyrometer) lost absolutely nothing of its weight. It is remarkable, also, that the pure native salt, if free from adventitious moisture, may be suddenly and strongly heated, with scarcely any of that sound called *decrepitation*,* which is produced by the similar treatment of all the varieties of artificial salt. Even these varieties, however, exposed during equal times to a low red heat, do not lose more than from half a grain to three grains in one hundred. This comparison cannot be extended to the salt prepared at a boiling temperature from sea water; because the muriate of magnesia, which

proportion of muriate of magnesia, that were discovered in the several varieties of Scotch salt, sent to me by Dr. THOMSON. For this reason, in stating the analysis of Scotch salt, I have given, in the table, that result which was most frequently obtained; and have withheld the names of the manufacturers, because the differences were probably in a great measure accidental, and not the result of greater or less skill in the preparation. One specimen of Lymington salt, which I examined, contained fully as much muriate of magnesia as any of the Scotch samples. The *cat salt* of that place, however, contrary to my expectation, proved to possess a very extraordinary degree of purity; a fact of which I satisfied myself by repeated experiments.

* Decrepitation is occasioned by the sudden conversion into vapour of the water contained in salts, when its quantity is insufficient to effect the watery fusion. It is a property peculiar to salts which hold only a very small proportion of water in combination, as muriate of soda, nitrate of lead, and sulphate of potash.

these varieties contain, is decomposed at a red heat, and deprived of its acid.

The following table shews the quantity of water contained in several kinds of salt, inferred from the loss which they sustain by ignition during equal times, after being first dried at 212°.

100 parts of large grained fishery salt contain of water		3
100	foreign bay salt (St. Martin's)	3
100	do. (Oleron)	2½
100	do. Cheshire common salt	1½
100	do. stoved salt	0½

The loudness and violence of the decrepitation was, as nearly as could be judged, in the same order, and was most remarkable in the large grained varieties.

To determine the proportions of real muriate of soda in those varieties of artificial salt which are nearly free from earthy muriates, I employed also the process of decomposition by nitrate of silver. The following are the quantities of fused *luna cornea*, obtained from 100 grains of each of three varieties dried, previously to solution, at the temperature of 212° FAHRENHEIT.

100 gr.	pure transparent rock salt gave of luna cornea	242
100	stoved salt, remarkably pure	239
100	fishery salt. do.	237*

* From 100 grains of pure artificial muriate of soda, previously heated to redness, Dr. MARCET has since informed me, that he obtained 24.16 grains of fused luna cornea. The weights of the precipitates thrown down in my experiments by nitrate of silver, are not, I am aware, exactly those which might have been expected from the table of the comparative proportions of water given in the text. Each experiment, however, was twice repeated with every precaution I could adopt, and with the same results. That different kinds of salt give different proportions of luna cornea, is

The proportion of ingredients in the several kinds of muriate of soda (setting apart the impurities) appears, therefore, to be nearly the same in all. And as the very minute quantity of water, discovered by analysis, is not constant in the several varieties, it may be inferred to be rather an accidental than a necessary ingredient; for in the latter case, an invariable proportion might be expected, conformably to the important law, establishing an uniformity in the proportions of chemical compounds, which has been explained by Mr. DALTON, and confirmed by Drs. THOMSON and WOLLASTON.

What then, it may be enquired, is the cause of those differences which are acknowledged, on all hands, to exist among the several species of muriate of soda, so far as respects their fitness for economical purposes. If I were to hazard an opinion, on a subject about which there must still be some uncertainty, it would be that the differences of *chemical composition*, discovered by the preceding train of experiments, in the several varieties of culinary salt, are scarcely sufficient to account for those properties, which are imputed to them on the ground of experience. The *stoved* and *fishery* salt, for example, though differing in a very trivial degree as to the kind or proportion of their ingredients, are adapted to widely different uses. Thus the large grained salt is peculiarly fitted for the packing of fish and other provisions, a purpose to which the small grained salts are much less suitable. Their different powers, then, of preserving food must depend on some mechanical property; and the only obvious one is the

proved also by comparing the experiment of Dr. MARCET, with the results of Dr. BLACK and KLAPROTH, both of whom found the fused muriate of silver, from 100 parts of common salt, to weigh 235 grains.

magnitude of the crystals, and their degree of compactness and hardness. Quickness of solution, it is well known, is pretty nearly proportional, all other circumstances being equal to the quantity of surface exposed. And since the surfaces of cubes are as the squares of their sides, it should follow that a salt whose crystals are of a given magnitude will dissolve four times more slowly than one whose cubes have only half the size.

That kind of salt, then, which possesses most eminently the combined properties of hardness, compactness, and perfection of crystals, will be best adapted to the purpose of packing fish and other provisions, because it will remain permanently, between the different layers, or will be very gradually dissolved by the fluids that exude from the provisions; thus furnishing a slow, but constant supply of saturated brine. On the other hand, for the purpose of preparing the pickle, or of *striking* the meat, which is done by immersion in a saturated solution of salt, the smaller grained varieties answer equally well; or, on account of their greater solubility, even better.

With the hardness or strong aggregation of the several varieties of salt, it seemed to me not improbable that their specific gravity might, in some degree, be connected. The exact determination of this property in saline substances is, however, a problem of considerable difficulty, as will sufficiently appear from the various results which have been given, with respect to the same salts, by different experimentalists. Thus MUSCHENBROEK makes the specific gravity of artificial muriate of soda to vary from 1918 to 2148, the mean of which is 2033. Sir ISAAC NEWTON states it at 2143, and HASSENFRATZ at 2200.*

* Annales de Chimie, Vol. XXVIII. p. 13.

All that was necessary for my purpose, was an approximation to the truth; and the introduction of a small error could be of no importance, provided it were the same in every case, since the comparison would still hold good.

The specific gravity of rock salt, there can be little difficulty in determining with precision. A piece of this salt,* of such perfect transparency, that I had reserved it as a cabinet specimen, weighed in the air 513 grains, and lost, when weighed in alcohol, 194 grains. The alcohol, at the temperature of 56° FAHRENHEIT, had the specific gravity of 820, and hence that of the salt may be estimated at 2170. Another specimen considerably less pure, and more approaching to a fibrous fracture, had the specific gravity of 2125 only.

For ascertaining the specific weights of artificial varieties of salts, I used a very simple contrivance. It consisted of a glass globe about 3½ diameter, having a stem or neck 10 inches long. Sixteen cubic inches of water (each 252½ grains at 60° FAHRENHEIT,) filled the whole of the globe, and about half an inch of the lower part of the neck; and from the line where the water stood in the instrument, it was accurately graduated upwards into hundredth parts of a cubical inch. Into this vessel I poured exactly sixteen cubic inches of a perfectly saturated solution of common salt; and then added 400 grains of the salt under examination, washing down the particles that adhered to the neck by a portion of the liquid, which had been previously taken out of the globe for the purpose. As much as possible of the air which adhered to the salt, was dislodged by agitation; and the increase of bulk was then observed.

Care was taken that the salts were all of equal temperature,

* Foliated rock salt of JAMESON. See his Mineralogy, Vol. II. p. 10.

and dryness, and that no change of temperature happened during the experiment.

	Hundredths of a cub. in.	Hence its specific grav. was*
400 grains of the less pure kind of rock salt, broken down into small fragments, filled the space of - - - -	75	2112
400 grains of stoved salt - - - -	75	2112
400 do. (another sample) - - -	76	2084
400 do. common salt - - -	76	2084
400 large grained fishery salt - - -	83	1909
400 do. (another sample) - - -	83	1909
400 St. Ube's - - - -	82	1932

If the above mode of determination at all approach to correctness, it would appear that the specific gravity of rock salt is diminished, by being broken into small fragments, from 2125 to 2112, probably in consequence of the quantity of air which the fragments envelope, and which cannot be entirely separated by agitation. From the numbers given in the last column, it is evident, that the smaller grained salts are specifically heavier than those which are composed of larger and more perfect crystals. A difference of only one or two hundredth parts of a cubic inch, is perhaps entitled, in a process of this kind, to little reliance; and I do not therefore regard it as indicating any material difference in the specific gravity of the first four, or last three salts submitted to experiment. But when the difference amounts to eight hundredths, as between the small and large grained salt, it may safely be imputed to an inferior specific gravity in that species, which occupies so much greater a proportional bulk.†

* Distilled water at 1000 being taken as the standard.

† M. Hassenfratz seems to have suspected that a difference in the specific

The last series of experiments proves decisively, that in an important quality, (viz. that of specific gravity,) which is probably connected with the mechanical property of hardness and compactness of crystals, little or no difference is discoverable between the large grained salt of British, and that of foreign manufacture. If no superiority, then, be claimed for British salt as applicable to economical purposes, on account of the greater degree of chemical purity which unquestionably belongs to it, it may safely, I believe, be asserted that the larger grained varieties are, as to their mechanical properties, fully equal to the foreign bay salt. And the period, it may be hoped, is not far distant, when a prejudice (for such, from the result of this investigation, it appears to be,) will be done away, which has long proved injurious to the interests and prosperity of an important branch of British manufacture.

SECTION III. ACCOUNT OF THE METHODS OF ANALYZING THE SEVERAL VARIETIES OF MURIATE OF SODA.

The method of analysis which I adopted, in examining the several varieties of muriate of soda, was as follows.

When the salt was in a state of solution, a measured quantity was evaporated to dryness in a sand heat, which was carefully regulated, to avoid the decomposition of the muriate of magnesia, if any of that salt were present in the solution.*

gravity of the same salt may be occasioned by a variation in its state of crystallization. *De la Pesanteur spécifique des Sels*, Ann. de Chem. XXVIII. p. 17.

* Muriate of magnesia, according to Dr. MARCET, begins to part with its acid at a temperature a few degrees above that of boiling water. This fact explains the observation of Mr. KIRWAN, that too great a heat, employed in the desiccation of muriate of magnesia, decreases considerably its solubility in alcohol. (KIRWAN on Mineral Waters, p. 275.)

Each specimen of salt was reduced to a fine powder, and was dried, in the temperature of 180° of FAHRENHEIT, during the space of two hours. This was done in order that the different experiments might be made on precisely equal quantities of salt.

I. *To separate the earthy Muriates.*

(A) On 1000 grains of the dried and pulverized salt, (except in the case of the foreign salts, when only 500 grains were used,) four ounce measures of alcohol were poured, of a specific gravity, varying from 815 to 820, and at nearly a boiling temperature. To insure the access of the fluid to every part of the salt, they were ground together for some time in a mortar, and then transferred into a glass mattras, where they were digested for some hours, and frequently agitated. The alcohol was next separated by filtration, and the undissolved part was washed, as it lay on the filtre, with 4 ounce measures of fresh alcohol.

(B.) The united washings were evaporated to dryness,* and to the dry mass a small portion of fresh alcohol was added, to separate the earthy muriates from a little common salt, which had been dissolved along with them. This solution might, however, still contain a minute portion of muriate of soda. It was therefore again evaporated, redissolved in hot water, and mixed with a solution of carbonate of soda. By boiling for some minutes, the whole of the earths were precipitated, and after being well washed, were re-dissolved in muriatic acid. This solution, being evaporated to dryness, gave the

* In this and all similar cases, the heat was very cautiously regulated towards the close of the process.

weight of the earthy muriates, which had been extracted by alcohol.*

(B. a.) The dry mass thus obtained might consist either of muriate of magnesia, of muriate of lime, or of both. An aliquot part, therefore, was dissolved, separately, for the purpose of assaying it by the usual tests. Sometimes, as in the case of the earthy muriates procured from sea salt, muriate of magnesia alone was indicated, and any further process was rendered unnecessary. Muriate of lime was in no instance found uncombined; but in the majority of cases (as in the earthy muriates obtained from Cheshire salt) was mixed with muriate of magnesia.

(B. b.) To the solution of two earthy muriates was added fully saturated carbonate of ammonia, which has the property of throwing down lime in combination with carbonic acid, but has no effect on the muriate of magnesia at ordinary temperatures. The solution of the latter salt, along with that of the excess of carbonate of ammonia, was therefore separated by filtration; and to the filtred liquor a solution of phosphate of soda was added, according to the formula of Dr. WOLLASTON.†

* By the analysis of artificial mixtures of pure muriate of soda with the earthy muriates in known quantities, I afterwards found that the full amount of the earthy muriates was not ascertained in this way of proceeding. The deficiency of the latter salts was about one sixth; but as the error must necessarily have been the same in all, it does not affect the comparison of different varieties of salt, as to their proportion of this ingredient. If the numbers in the 5th column of the table (indicating the total earthy muriates) be increased in the proportion of six to five, we shall then obtain the true quantities in each variety of salt.

† See Dr. MARCET's analysis of the Brighton Chalybeate, published in the last edition of SAUNDERS on Mineral Waters.

(B. c.) By direct experiments I had learned that 100 grains of muriate of magnesia, when thus decomposed by carbonate of ammonia, conjoined with phosphate of soda, give 151 grains of an insoluble ammoniaco-magnesian phosphate dried at about 90° of FAHRENHEIT. Hence it was easy, from the weight of the precipitate, to calculate how much of the former salt was contained in the mixture of muriate of lime and muriate of magnesia. Thus, if 20 grains of a mixture of the two muriates yielded 15.1 of ammoniaco-magnesian phosphate, it is obvious that the mixture must have consisted of equal weights of muriate of lime and muriate of magnesia.

(B. d.) The estimation of the proportion of muriate of lime, in a mixture of this salt with muriate of magnesia, was sometimes performed in a different way. To a cold solution of a known weight of the two salts, super-oxalate of potash was added; and the precipitate was collected, washed, and dried at about 160° FAHRENHEIT. Of this precipitate I had previously found that 116 grains are formed by the decomposition of 100 grains of dry muriate of lime. From the quantity of oxalate of lime it was easy, therefore, to infer that of the muriate, from whose decomposition it resulted; and this subtracted from the weight of the two salts, gave the weight of the muriate of magnesia.

II. *To separate and estimate the earthy Sulphates.*

(C.) The portion of salt which had resisted the action of alcohol, was dissolved by long boiling in sixteen ounce measures of distilled water, and the solution was filtered. On the filtre a small quantity of undissolved matter generally remained, which was washed with hot water, till it ceased to

have any action. The weight of the insoluble portion was then ascertained.

(C. a.) By this operation were dissolved, not only the muriate of soda, but all the other salts, insoluble in alcohol, which might be mingled with it. To the solution, carbonate of soda was added; and the liquid, which in most cases gave, on this addition, an abundant precipitate, was boiled briskly for several minutes, in order that none of the earthy carbonates, which were separated, might remain dissolved by an excess of carbonic acid.

(C. b.) The precipitated earths were allowed to subside, and were well edulcorated with boiling water, the washing being added to the liquor first decanted from the precipitate. To these united liquids, (after the addition of more muriatic acid than was required for saturation) muriate of barytes was added, till it ceased to occasion any further precipitate. The sulphate of barytes was then washed sufficiently; dried; ignited; and its amount ascertained.

To the earthy carbonates, an excess of sulphuric acid was added in a platina dish, and the mixture was triturated, till all effervescence ceased. It was then evaporated to dryness, calcined in a low red heat, and the weight of the earthy sulphates was ascertained.

(D. a.) The dry sulphates were washed with a small quantity of lukewarm water. In several instances, the loss of weight, thus sustained, was extremely trifling, nothing being dissolved but a very minute portion of sulphate of lime, of which earthy salt, solely, the residue was presumed to be composed.

(D. b.) But in other cases, a considerable loss of weight

ensued; and in these, to the watery solution was added a mixture of equal parts of saturated solutions of carbonate of ammonia, and phosphate of soda. A precipitate more or less copious was produced, which was collected, dried at 90° FAHRENHEIT, and weighed.

(D. c.) By direct experiments I had determined, that 90 grains of this precipitate result from the decomposition of 100 grains of sulphate of magnesia, of such a degree of dryness, as to lose 44 grains out of 100, by exposure to a low red heat. Hence 100 grains of ammoniaco-magnesian phosphate indicate 111 grains of crystallized, or 62.2 of desiccated, sulphate of magnesia.* From the weight of the ammoniaco-magnesian phosphate, it is easy, therefore, to infer the proportion of sulphate of magnesia in any mixture of the two earthy sulphates.

(D. d.) It was possible, however, that in addition to the sulphates of lime and of magnesia, the quantity of which had been determined by the foregoing process, the specimen of salt under examination might contain also an alkaline sulphate. To decide this point, it was necessary to compare the amount of the acid, deducible from the weight of the sulphate of barytes (C. b.), with that which ought to exist in the sulphate of lime, and sulphate of magnesia actually found by experiment. But to make this comparison, some collateral experiments were previously necessary.

(D. e.) By these experiments, I found that sulphate of lime prepared by double decomposition, then calcined in a low

* The assumption that crystallized sulphate of magnesia contains only 44 per cent. of water, though it was correctly true with the specimen on which I operated, is below the average, which, I find from several experiments, is about one half the weight of the salt. Mr. KIRWAN states the water of crystallization to be 53.6 in 100 grains; but this, I believe, a little exceeds the truth.

red heat, and afterwards dissolved in a large quantity of boiling distilled water, yields, when precipitated by a barytic salt, in the proportion of 175.9 grains of sulphate of barytes from 100 of the calcareous sulphate.* The same quantity of ignited sulphate of lime (= 128 grains dried at 160° FAHRENHEIT,) precipitated by super-oxalate of potash, gives 102.5 of oxalate of lime; or, precipitated by sub-carbonate of potash at a boiling heat, 74.3 grains of carbonate of lime.† One hundred grains of crystallized sulphate of magnesia (= 56 desiccated) afford when precipitated by muriate of barytes, 111 or 112 of the barytic sulphate.

(E.) By a comparison of the above proportions with those obtained in the analysis of any specimen of common salt, we may learn whether it contain other sulphates beside those with earthy bases. For example, if the precipitate (D.) consist of carbonate of lime only, and bear to the sulphate of barytes (C. b.) the proportion of 74 to 175, or very nearly so, we may infer, that no other sulphate is present, but that of lime. The same conclusion will follow, if, after having decomposed one half of the watery solution (C.) by muriate of barytes, and another half by oxalate of potash, we find that the sulphate of barytes bears to the oxalate of lime, the proportion of 175.9 to 102.5.

* This result corresponds, within a fraction of a grain, with one obtained in a somewhat different way by Dr. MARCET, and very nearly with an experiment of my friend Mr. JAMES THOMSON, who found the barytic sulphate, precipitated from 100 grains of sulphate of lime by nitrate of barytes, to weigh 173 grains.

† On reversing this experiment, I found that 100 grains of carbonate of lime, saturated with sulphuric acid, and calcined in a low red heat, afford 135 of sulphate of lime. A similar experiment of Mr. THOMSON gave 134.6 grains. Dr. MARCET, also, informs me that from 93.53 grains of pure marble, he obtained 125.95 grains of sulphate of lime, proportions which exactly co-incide with those of Mr. THOMSON.

Now these proportions were, as nearly as could be expected, obtained in the analysis of Northwich salt; from whence we may conclude, that the only sulphate which it contains, is gypsum, or the sulphate of lime.

It must be remembered, however, that the calcareous sulphate, contained in any variety of common salt, cannot be in a state of complete desiccation, but would lose 22 parts out of 100, by exposure to a red heat.* It becomes necessary, therefore, either to increase, in the proportion of 5 to 4, our estimate of the sulphate of lime obtained by the foregoing rule, or, more simply, to assume that 100 grains of sulphate of barytes, indicate 78 grains of sulphate of lime, dried at 160° FAHRENHEIT, = 57 ignited.

(F.) When sulphate of lime and sulphate of magnesia, were both ascertained, and other sulphates also might possibly be present, as in the varieties of salt from sea water, the calculation became a little more complicated. In this case, after determining the quantity of both sulphates, (by the processes D. &c.) I estimated how much sulphate of barytes they ought respectively to afford; and then compared the estimated quantity, with that which was actually obtained. The earthy carbonates, for example, precipitated from 1000 grains of Lymington salt, which had previously been digested with alcohol, were converted into 31 grains of calcined sulphates, consisting of 19 grains of dry sulphate of magnesia, and 12 grains of dry sulphate of lime. Now from the magnesian sulphate 38 grains of sulphate of barytes should result, and

* This I find to be the loss sustained by 100 grains of artificial selenite, dried at 160°, and then ignited. The same quantity of crystallized native selenite, I learn from Dr. MARCET, loses 20.7 grains, by being calcined in a strong red heat.

from the sulphate of lime, 21 grains, the sum of which is 59. But the quantity actually obtained was 59.8. There is only, therefore, an excess of 0.8 grain of the actual above the estimated quantity, a difference much too trivial to be admitted as an indication of any sulphate with an alkaline base; and arising, probably, from unavoidable errors in the experiment.

(F. a.) If in any mixture of salts, free from the earthy muriates, we are certain that no other sulphates exist beside those of lime and magnesia, their estimation becomes extremely simple. Decompose two equal quantities of the salt in question, the one by muriate of barytes, the other by oxalate of potash. From the weight of the latter precipitate, we may calculate the quantity of sulphate of lime. Suppose, for example, the oxalate of lime (as was actually the case with the precipitate from 1000 grains of Lymington salt,) to weigh 12 grains; these denote 15 of sulphate of lime, dried at 160° FAHRENHEIT, which quantity, if decomposed, would give $20\frac{1}{2}$ of sulphate of barytes. The latter number ($20\frac{1}{2}$), subtracted from the weight of sulphate of barytes actually obtained (say 60), gives $39\frac{1}{2}$ grains for the sulphate of barytes, resulting from the decomposition of sulphate of magnesia. The quantity of the latter salt, it will be found, therefore, by applying the rule already given (D. e.), must be 35 grains.

(F. b.) The same object may be accomplished by decomposing two equal quantities, the one by oxalate of potash, the other by the compound solution (D. c.). From the weights of the precipitates, it is easy to calculate from how much of the calcareous and magnesian sulphates they have resulted.

(G.) When the salt left by alcohol was known to contain muriate of soda, and sulphate of magnesia, but no sulphate of

lime, the presence of alkaline sulphates was investigated in the following manner. The salt was dissolved in water, and the solution was divided into two equal portions. To the one muriate of barytes was added, and to the other, the compound precipitant of carbonate of ammonia, and phosphate of soda. If the sulphate of barytes, thus produced, bore to the ammoniaco-magnesian phosphate, the proportion of 112 to 90, it was concluded that no other sulphate had been decomposed, but that with base of magnesia.

(H.) At one time, I expected to have ascertained the quantity of sulphate of soda, in an artificial mixture of that salt with sulphate of magnesia and muriate of soda, by the following formula. To a solution of the three salts, heated to a boiling temperature, I added sub-carbonate of ammonia, which decomposes the sulphate of magnesia only. I had then a solution containing muriate and sulphate of soda, with sulphate of ammonia, and some carbonate of ammonia. This solution was evaporated to dryness, and the mass was sufficiently heated to expel the ammoniacal salts. I found, however, that at this temperature, the sulphate of ammonia acted upon the muriate of soda, and produced an additional, and not inconsiderable quantity of sulphate of soda.

Having determined, by the foregoing processes, the quantity and kind of the earthy muriates, the amount of the insoluble matter, and the proportion of sulphates, the weights of all these different impurities were added together, and the sum being deducted from the weight of the salt submitted to experiment, the remainder was assumed as the amount of the pure muriate of soda in the specimen under examination.*

* I have deemed it unnecessary to state, in the table, the quantities of acid and

Though I purposely refrain from giving the details of the several analyses, which were made according to the foregoing plan, from the conviction that they would be both tedious and unnecessary, yet there are a few circumstances, which it may be proper to mention more fully, than can be done in the form of a table.

1. The *brine* which I examined was from Northwich, and was sent to me in the state in which it was taken from the spring.* At the temperature of 56° FAHRENHEIT, it had the specific gravity of 1205. It was perfectly limpid, but lost a little of its transparency when raised to a boiling heat, in consequence of the deposition of a very minute quantity of carbonate of lime, and oxide of iron. It was immediately precipitated by muriate of barytes, oxalate of ammonia, and alkaline solutions, both mild and caustic. Eight ounce measures, evaporated to dryness in a sand heat, gave 1930 grains of salt, which, for the sake of distinction, I term *entire* salt. It proved, on analysis, to contain in one thousand parts,†

base in the several varieties of muriate of soda. They may readily be estimated from the proportion, deduced by Dr. MARCET, of 46 acid, and 54 soda, in 100 of the pure muriate. In this determination he assumes, that 100 parts of luna cornea, after being melted and heated to redness, consist of 19.05 parts of acid, to 80.95 oxide of silver. This statement agrees very nearly with the recent one of GAY LUSSAC, who makes 100 parts of silver to combine with 7.60 oxygene, and this oxide to neutralize 25.71 parts of real muriatic acid.

* I have lately been informed that this brine had been pumped out of a rock-salt mine, into which, from the impossibility of obtaining the salt in a solid form, it was allowed to flow. Hence it was fully saturated with muriate of soda.

† The specific gravity and proportion of earthy sulphates in Cheshire brine appears to differ considerably in the brine of different springs. See HOLLAND's Cheshire Report, p. 45, &c.

Carbonate of lime and oxide of iron	2
Muriate of lime, and muriate of magnesia, in nearly equal proportions	5
Sulphate of lime	19
Muriate of soda	974
	1000

2. The *mother liquor*, or brine that remains after separating all the common salt, which it is thought worth while to extract, had the specific gravity of 1208. The dry salt contained

Muriate of magnesia	35
lime	32
Sulphate of lime	6
Muriate of soda	927
	1000

3. The *clearings* of the brine, which are raked out of the pan when the salt first begins to granulate, contained in 1000 parts,

Muriate of soda	800
Carbonate of lime	41
Sulphate of lime	159
	1000

4. Of the substance called by the workmen *pan-scale*, two specimens were analysed, the one containing a large proportion of muriate of soda, the other very little. The first variety consisted of

Muriate of soda	950
Carbonate of lime	10
Sulphate of lime	40
	1000

The second variety was composed of

Muriate of soda - -	100
Carbonate of lime - -	110
Sulphate of lime - -	790
	1000.

Circumstances, however, are constantly occurring to vary the proportion of ingredients, both in the clearings and in the pan-scale. If, for example, the brine be short of the point of saturation with common salt, it acts, when admitted into the pan, upon the muriate of soda which the pan-scale contains, and we obtain the second variety. But if the brine be fully charged with salt, it effects no solution of the muriate of soda, carried down along with the gypsum; and then the first species of pan-scale results.

5. The *salt oil*, or mother liquor from sea water, a specimen of which I received from Dr. THOMSON, had the specific gravity of 1277. It was abundantly precipitated by muriate of barytes: by pure ammonia, but not by the carbonate; and was not changed by oxalate of potash, either immediately or after an interval of some hours. One thousand parts of the dry salt consisted of

Muriate of magnesia - - -	874
Sulphate of magnesia - - -	70
Muriate of soda - - -	56
	1000.

6. The *salt brine*, or liquor which drains from the Scotch salt, had the specific gravity of only 1188. It was affected by the same tests as the salt oil, but less remarkably. The dry residue contained

Muriate of magnesia - - -	205
Sulphate of magnesia - - -	135
Muriate of soda - - - -	660
	1000.

7. The *mother liquor*, or *bittern pan Lymington*, presented, on analysis, an unaccountable variation from the similar fluid sent from Scotland, and gave a much larger proportion of sulphate of magnesia. A considerable quantity of this salt had, moreover, crystallized in the bottle which contained the liquid. Its specific gravity was 1280. One thousand parts of the dry salt contained of

Muriate of magnesia - - -	640
Sulphate of magnesia - - -	260
Muriate of soda - - -	100
	1000.

8. The pan-scale from Lymington contained

Muriate of magnesia - - -	29
Desiccated sulphate of magnesia - -	18
Carbonates of lime and magnesia* - -	127
Sulphate of lime - - -	216
Muriate of soda - - -	610
	1000.

From the very near approximation of the proportions between the sulphate of barytes and ammoniaco-magnesian phosphate, obtained in the analysis of all these products of sea water, to those which result from the decomposition of

* The proportion of these carbonates I was prevented from determining by an accident.

two equal quantities of sulphate of magnesia, it may be inferred that they contain no sulphate of soda.* For example, to decide whether the Scotch salt contains an alkaline sulphate, or not, I dissolved 1500 grains in a pint of boiling water, and evaporated till fourteen drachm measures only remained, the common salt being removed as soon as it was formed. The residuary liquid was divided into two equal portions, one of which gave 18½ grains of sulphate of barytes, and the other, 14 grains of ammoniaco-magnesian phosphate. The proportion between these numbers is so nearly that which has been already assigned, (viz. 112 to 90,) that we may safely infer the total absence of sulphate of soda. This salt, indeed, is considered as incompatible with muriate of magnesia; but after digesting, for two or three days, 100 grains of the former, with 90 of the latter, evaporating to dryness, and washing the residuum with repeated affusions of alcohol, I found that two grains of the muriate of magnesia had escaped decomposition.

* I employed more attention in investigating the presence of sulphate of soda in the products of sea water, because this salt is stated to be one of its ingredients by the Bishop of LLANDAFF, (Chemistry vol. ii. p. 62,) and by other chemical writers.

Manchester,
June 19, 1809.

V. *Description of an extraordinary Human Fœtus. In a Letter from Mr.* Benj. Gibson, *Surgeon, to* H. Leigh Thomas, *Esq. F. R. S.*

Read February 8, 1810.

Manchester, June, 1809.

Dear Sir,

I have lately had an opportunity of inspecting a singular human fœtus, at the full period of utero-gestation, and hope that a history of it will not be unacceptable to the learned Society, of which you are a member. Some deviations, which it exhibits, from the usual form and structure of the human body, have not (as far as my information extends) been yet recorded; and the consideration that they were all apparently compatible with life, gives additional interest to the subject; for if the difficulty of parturition had not proved almost immediately fatal, the complex structure of the animal would have formed no impediment to its existence.

Instances of two entire human bodies united together, are by no means rare in the collections of anatomists. Sometimes their conjunction takes place back to back; sometimes the reverse; at other times side by side; or in such a position as to form a kind of cross: the two heads lying in one direction, and the opposite extremities in another, and intersecting it at right angles.

That species of monstrosity, however, in which two bodies are merely united together, does not display in a striking

point of view, the resources of nature in accommodating her means, or modifying her materials to any new situation. All that is remarkable, is the conjunction of two bodies complete in themselves: the organs which compose them have their ordinary structure and arrangement; the anatomist or physiologist anticipates nothing curious in their internal configuration. It is principally in those instances of unnatural deviation, where some parts of the body are formed double, and others single, that the resources of nature become apparent, in adjusting parts, which, in the natural state of the body, have no adaptation.

Of this kind is the singular fœtus, of which I shall now proceed to offer the description.

This curious production was formed with two heads placed side by side, united apparently to one body, with two legs and two arms. The one head expressed the character of a male, the other evidently that of a female. This distinction of sex, conspicuous enough from mere inspection, was still corroborated by the conformation of the organs of generation. From external examination it was plain, that there were two spines corresponding to the two heads; these were found to terminate in a double os sacrum, tipped with two ossa coccygis. The superior part of the spines diverged considerably, so that when the two faces were turned towards each other, the lips came in contact. Above the first lumbar vertebra, they approached each other, still however forming two independent vertebral canals, for the passage of the spinal marrow from each head. In consequence of the divergence of the spines at their superior part, the chest was formed much broader than natural. The common number of ribs were placed on the

outside of each spine; and between the inner sides of the spines, occupying the space formed by their divergence, was interposed an equal number of somewhat angular ribs, of about an inch in length. These were common to both spines, and possessed a considerable latitude of motion, but were unconnected with any bone similar to the sternum. These short ribs, by occupying the situation of the spine in the natural state of the body, completed the posterior part of the chest.

The only remaining peculiarities in the osseous system, was a third scapula, small in size, and placed at the root of each neck, upon the small ribs; and a third clavicle, inserted at one extremity into the glenoid cavity of this scapula, and attached by the other to the upper part of the sternum.

Before I proceed to describe the conformation of the vascular system, and the distribution of the nerves of the thoracic and abdominal viscera, it will be necessary, first to notice the arrangement, connection, and number of these viscera.

The chest was divided by the mediastinum into two cavities, containing, amongst other organs, the lungs, and two hearts, placed contiguous to each other. It was ascertained by inflation, that the lungs on the right side were entirely independent of those on the left, each being supplied with air by a distinct trachea, from the corresponding head. They were not divided into lobes in the usual way: on the right side there were only two lobes, on the left only one. The only peculiarity with respect to the diaphragm was, that instead of being attached to the spine and lower ribs, on the right side of the body, it crossed over a considerable portion of the right lobe of the liver; this muscle, therefore, assisted in forming a cavity, which was situated upon the two spines,

and descending aorta, and underneath the apex of each heart, and the large vessels, which entered the right heart from the liver. In this cavity, which may be considered as a second abdomen, were deposited, a spleen, pancreas, omentum, and stomach, connected with the œsophagus of the right head. It communicated with the usual cavity of the abdomen, by an opening similar to the foramen of WINSLOW; for a probe, introduced underneath the vessels of the liver, passed into it.

Little irregularity was observable in the viscera of the abdomen. A second stomach occupied nearly its usual situation. With this the duodenum was connected in the common way; and at a short distance from the pylorus, was joined by the duodedum from the stomach placed in the cavity of the chest. Close to this junction, the ductus communis choledochus, and ductus pancreaticus, entered the intestine. The only circumstance of irregularity in the other intestine was, that the cœcum and that part of the colon, which is, generally, closely bound down by the peritoneum to the right side, was attached to it by a membrane of considerable length, which allowed them to be turned to the right or left side indifferently. All the intestines, however, were more capacious than natural.

As I anticipated some remarkable deviations from nature in the formation and arrangement of the vascular system, I carefully injected the body by the umbilical vein. On inspecting the chest, I found two hearts, formed of the usual number of auricles and ventricles, and inclosed in separate pericardia. The heart situated towards the left side, and corresponding to the male head, was considerably the largest. From its ventricles the aorta and pulmonary artery arose in

the usual way, and communicated with each other by a ductus arteriosus. The blood conveyed by the pulmonary artery passed to the lungs situated on the left side of the chest. From the arch of the aorta, the two carotid arteries were sent off in one trunk, which soon divided. The vertebral and inferior thyroid arteries, with the left axillary artery, were detached from the aorta, after it had almost completed its arch, and had received the ductus arteriosus. This aorta soon afterwards united with the aorta of the right heart, and descended as one trunk, embedded in the hollow between the two spines, and resting upon the short ribs, which were connected with them.

With regard to the veins which entered this heart, those from the lungs terminated in the left auricle, in the usual way. The vena cava superior terminated in the right auricle, and had this peculiarity, that it not only received the blood from the jugular veins of the left head, and from the left subclavian vein, but from the right subclavian vein also, and from the jugular veins of the right head. There was also a large vessel, which will be noticed afterwards, forming a communication between the vena cava superior of this heart (the left), and the vena cava inferior of the right heart. The principal peculiarity, however, of this heart consisted in a partial defect of the vena cava inferior. A large vein, which entered the lower part of the right auricle, was formed by the venæ cavæ hepaticæ, and two ductus venosi, which were smaller than natural.

The heart situated towards the right side of the chest differed in many respects from that on the left, both in the relative situation of the vessels which arose from it, and in

three vessels which were deficient. The position of its auricles and ventricles was peculiar, and the whole heart was in a great measure reversed. The two auricles lay in contact, and were not separated by the origin of the large vessels. The ventricle situated towards the right side of the body gave off the aorta; whilst the pulmonary artery arose from the left ventricle, which was contiguous to the heart of the left side. These vessels were united by a ductus arteriosus. The pulmonary artery, however, was not situated before the aorta at its origin; it passed behind the superior part of the right ventricle, so as to get behind the origin of the aorta, where it was divided into branches, distributed to the lungs of the right side of the chest. At this point also, the ductus arteriosus was detached from the pulmonary artery, and following the direction of the aorta upwards, appeared like a second arch, placed behind the arch of the aorta. In fact, the arch formed by the ductus arteriosus very nearly resembled that of the aorta, by sending off a large vessel from its convex side.

From the arch of the aorta the carotid arteries were given off, as distinct branches, but no axillary artery for the arm of the right side. This vessel, with the right vertebral, and the inferior thyroid arteries of the right head, were detached from the middle of the ductus arteriosus in one trunk, which soon divided into three branches. This aorta then descended a little, and united to its fellow, from the heart of the left side.

I shall pass over the smaller vessels derived from the descending aorta, and only notice, that in the situation of the cæliac artery, three or four vessels were given off, crowded together: they appeared to be destined for the liver, and for the two stomachs, two spleens, and single pancreas.

The principal deviation from the natural structure in the venous system of this heart, consisted in the want of the vena cava superior. The vena cava inferior might be said to be natural. It received the veins from the lower extremities, from one kidney, &c. and also the branches of the venæ cavæ hepaticæ, which did not terminate in the opposite heart. It received also a ductus venosus from the umbilical vein. The only remaining circumstances with respect to the distribution of the blood vessels, were two very large anastomoses between the branches of the vena portarum; and a remarkable branch of communication, between the lower part of the vena cava inferior of the right heart, and the vena cava superior of the left heart. This communicating vessel was detached from the vena cava inferior, opposite to the right emulgent vein, and, having crossed over the anterior part of the aorta, below the origin of the cæliac artery, received the left emulgent vein. It then proceeded upwards, and terminated in the vena cava superior. Although this vessel was situated on the left side of the aorta, it probably served the office of the vena azygos.

Although I have already noticed several instances of curious conformation, that of the organs of generation was in some respects the most remarkable. From the features of the two heads I was led to expect both male and female parts, as the countenances clearly expressed the two sexes. In these organs the male character was predominant; the penis was of good size: the testes in the act of passing into the scrotum: the vesiculæ seminales were perfect and well formed. Still these parts partook in some measure, of the conformation of the female organs. The glans penis was formed precisely like that of the clitoris; was covered by a similar prepuce, without

a frænum. It was also imperforate, like that of the clitoris; for although the urethra was continuous with the penis, it did not perforate the substance of the glans; but, for a considerable extent, passed on the under surface of the penis, merely covered by the common integuments: nor was it surrounded by the corpus spongiosum, till it arrived nearly as far as the bulb of the urethra. These were all the female characters I observed, till being induced to open the bladder, which felt uncommonly thick and muscular, I discovered a uterus placed in its cavity, in some measure incorporated with the substance, and forming a part of that viscus. It was situated towards the most depending part of the bladder; its fundus projected about half an inch, whilst the rest of its body and cervix, was incorporated with the posterior part of the bladder. From its superior part, the fallopian tubes proceeded, beautifully convoluted; they penetrated through the substance of the bladder, and, as far as I could ascertain, terminated in a cul de sac near the vesiculæ seminales. The uterus was hollow; its cervix extended to the beginning of the urethra, where it terminated by an oval opening on the prostate gland, nearer to the neck of the bladder than the verumontanum. I could not discover whether the ovaria were connected with the uterus, but it is extremely probable, that they were situated exterior to the bladder, near the termination of the fallopian tubes, and might have been removed accidentally, during the dissection of parts, where their existence was little expected.

As I had no reason to believe that there was any variety in the distribution of the nerves of the extremities, my attention was principally directed to those of the thoracic and abdominal viscera.

The eighth pair of nerves had its natural origin from each brain, and gave off the usual branches in passing down the neck. In the chest, the pair derived from the right head supplied the right heart, lungs, stomach, &c. The pair from the left head, was distributed in a similar manner to the organs of the left side. Branches were detached from each pair to the semilunar ganglion.

A pair of intercostal nerves proceeded also from each head. In describing them, it will be convenient to distinguish those which passed on the inside of each spine, by the name of inner intercostals; those on the outside of each spine, by that of outer intercostals. There was this peculiarity in the inner intercostal nerves, that they passed along, without giving off, or receiving communicating branches, or forming the usual ganglia, in consequence of the want of all the cervical, dorsal, and lumbar nerves, on the inner side of each spine, except the first cervicals. In the chest, the two inner intercostals formed a junction, and accompanied the descending aorta downwards. But before this joint nerve arrived at the semilunar ganglion, two considerable branches were detached, which continued to accompany the aorta, and were ultimately lost in the cavity of the pelvis. The greatest part of the nerve, however, terminated in the semilunar ganglion.

The outer intercostals, or those passing on the outside of each spine, had, as far as I could investigate them, their usual distribution.

I have already had occasion to mention the deficiency of all the cervical, dorsal, and lumbar nerves, on the inside of each spine; except the first cervicals, of course all the nerves, which are commonly derived from them, were wanting. Thus

there was only one pair of the phrenic nerves; one nerve on the outside of each neck, which was distributed to the diaphragm in the usual way. The other nerves from the spinal marrow were in like manner distributed, as if the body had had but one spine. Thus the nerves of the right arm and leg, were supplied by the right side of the right spinal marrow: those of the left arm and leg, by the left side of the left spinal marrow.

These are all the circumstances connected with this curious fœtus, which I think worthy your notice; and, having already sufficiently trespassed upon your patience, I subscribe myself

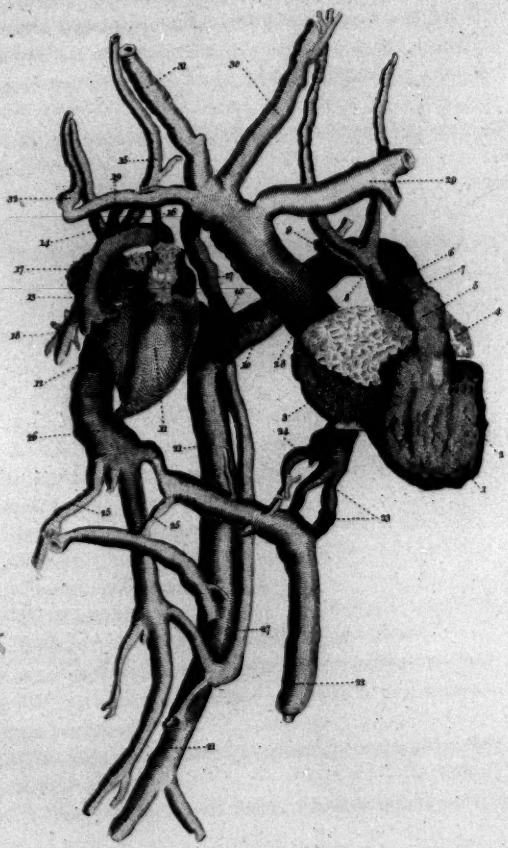
Your obliged Servant,

B. GIBSON.

EXPLANATION OF PLATE III.

Anterior View of the Heart and Arteries, removed from the Body.

1. The right ventricle of the heart, connected with the left head.
2. The left ventricle.
3. The right auricle.
4. The left auricle.
5. The pulmonary artery at its origin.
6. The ductus arteriosus.
7. The aorta.
8. The trunk, from which the two external carotid arteries of the left head arose.



P. Reeve sc.

9. The trunk, from which the vertebral, the inferior thyroid arteries, and the left axillary artery, arose.
10. The aorta after it has completed its arch, and is proceeding to unite with the aorta, from the heart of the right head.
11. The left ventricle of the heart, corresponding to the right head.
12. The right ventricle.
13. The aorta, arising from the right ventricle.
14. The right external carotid artery of the right head.
15. The left external carotid artery.
16. The left vertebral artery, arising from the arch of the aorta as a distinct vessel.
17. The pulmonary artery, where it detaches the ductus arteriosus.
18. Branches of the pulmonary artery.
19. A vessel detached from the ductus arteriosus, from which the right axillary, the right vertebral, and the inferior thyroid arteries, are derived.
20. The point of junction between the aorta of the right and left heart.
21. The descending aorta.
22. The umbilical vein.
23. Two ductus venosi detached to the left heart.
24. A trunk formed by the venæ cavæ hepaticæ.
25. Large anastomoses formed between the branches of the vena portarum.
26. The vena cava inferior, terminating in the left auricle of the right heart.
27. A large vessel, which forms a communication between

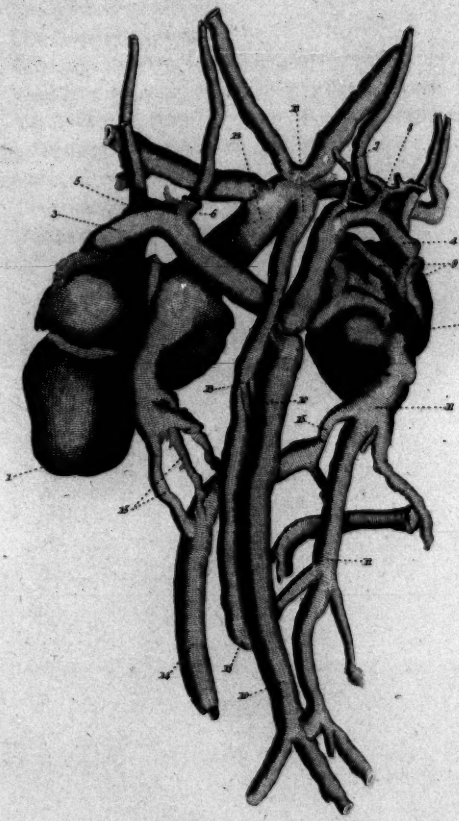
the vena cava inferior of the right heart, and the vena cava superior of the left heart

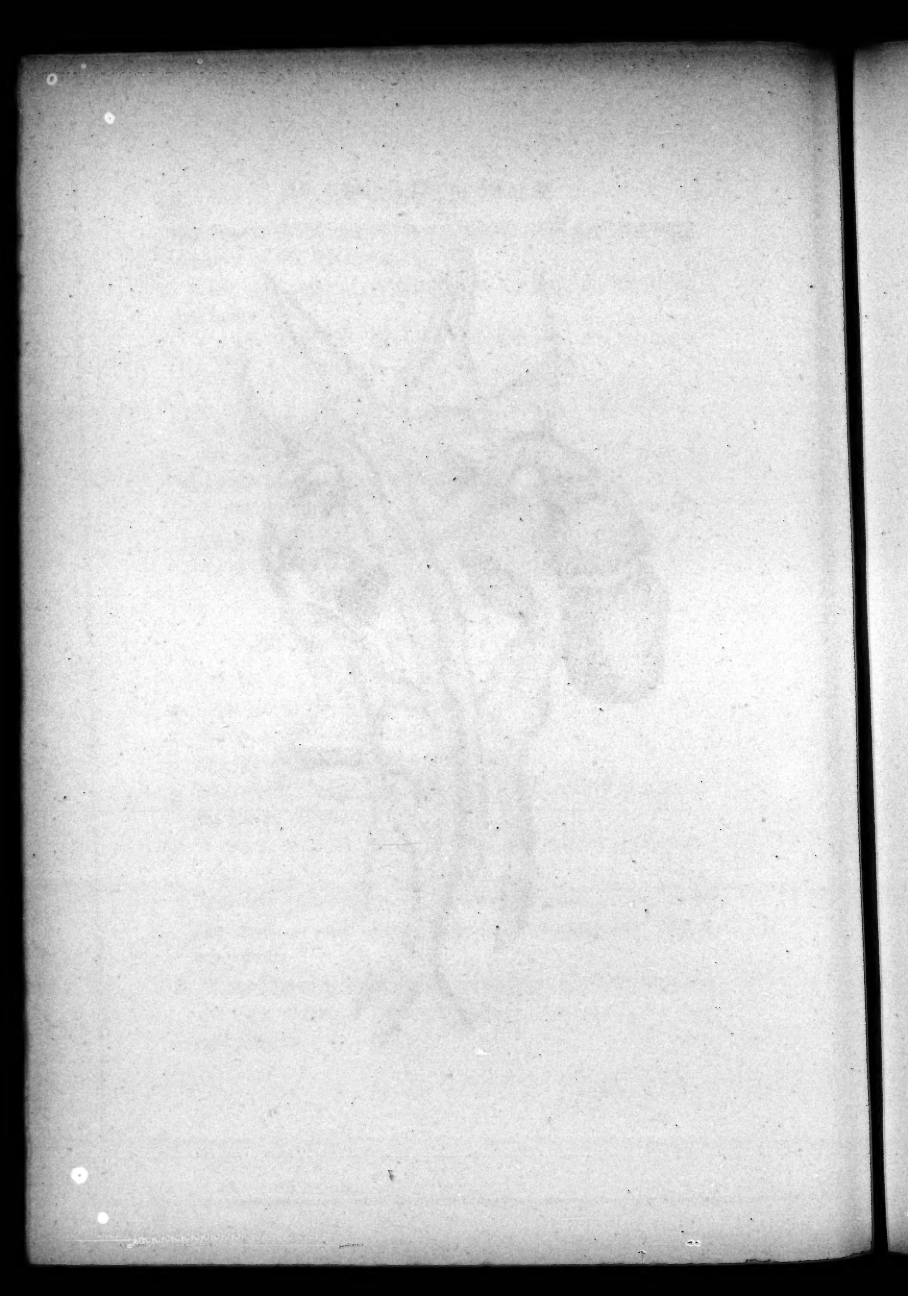
28. Vena cava superior, terminating in the right auricle of the left heart.
29. A trunk formed by the left subclavian vein, and the jugular veins of the left side of the left head.
30. A trunk formed by the jugular veins of the right side of the left head.
31. A trunk formed by the jugular veins of the left side of the right head.
32. A trunk formed by the right subclavian vein, and the jugular veins of the right side of the right head.

Explanation of Plate IV.

Posterior View of the Hearts and Arteries.

1. Left heart.
2. Right heart.
3. Ductus arteriosus of the left heart.
4. Ductus arteriosus of the right heart.
5. The arch of the aorta of the right heart, where it receives the ductus arteriosus.
6. A trunk, from which the vertebral and inferior thyroid arteries, with the left axillary artery, arose from the aorta.
7. The left vertebral artery of the right head, arising from the arch of the aorta, just as it receives the ductus arteriosus.
8. A trunk arising from the ductus arteriosus, which supplied the right vertebral, the right axillary, and the inferior thyroid arteries.





9. The pulmonary arteries.
10. The descending aorta.
11. Vena cava inferior, terminating in the right heart.
12. Vena cava superior, terminating in the left heart.
13. A considerable vessel of communication, passing between the vena cava superior of the left heart, and the vena cava inferior of the right heart.
14. The umbilical vein.
15. Ductus venosi.

VI. *Observations on the Effects of Magnesia, in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr.* William T. Brande, *F. R. S. to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.*

Read February 22, 1810.

Mr. Home's enquiries into the functions of the stomach, and his discovery of liquids passing from the cardiac portion, into the circulation of the blood,* led him to consider, that the generality of calculous complaints might possibly be prevented, by introducing into the stomach, such substances as are capable of preventing the formation of uric acid, and that this mode of treatment would have many advantages over the usual method, which consists in attempting to dissolve the uric acid after it is formed.

He consulted Mr. Hatchett on the substance most likely to produce this effect, and asked if magnesia, from its insolubility in water, was not well adapted for the purpose, as it would remain in the stomach, until it should combine with any acid, or be carried along with the food towards the pylorus.

Mr. Hatchett knew of nothing more likely to produce the desired effect; and on putting this theory to the test of experiment, it was found by a very careful examination of

* Philos. Trans, 1808.

the urine, that in several instances where there was an increased formation of uric acid, magnesia diminished it in a much greater degree than had been effected by the use, and that a very liberal one, of the alkalies in the same patient.

This circumstance led Mr. HOME to wish for a more complete investigation of the subject, and he requested me to assist him in the prosecution of it. Since that time many opportunities have occurred of carrying on the inquiry during an attendance on patients, labouring under calculous complaints.

It is proposed to lay the results of our joint labours before this Society, with a view to establish a fact of so much importance in the treatment of those diseases.

The four following cases, include the principal varieties of the disorder, which have been met with, and are therefore selected from among many others, to prevent unnecessary repetitions. In each of them the urine was occasionally carefully analysed.

CASE I.

A gentleman, sixty years of age, who had been in the habit of indulging in the free use of acid liquors, had repeatedly passed small calculi composed entirely of uric acid; his urine immediately after being voided, deposited at all times a considerable quantity of that substance, in the form of a red powder, and occasionally in larger crystals.

Nine drachms of subcarbonate of soda, dissolved in water highly impregnated with carbonic acid, and taken in the course of the day at three doses, appeared to have no effect whatever on the formation of uric acid; the red sand was

deposited as usual, and the small calculi continued to form.

On account of the inefficacy of this medicine, he was advised to try the vegetable alkali, and three drachms of subcarbonate of potash dissolved in water, slightly impregnated with carbonic acid, were taken at similar intervals.

The deposit of uric acid in the urine, was now somewhat diminished; but during this free use of alkalies, which with little interruption, was persevered in for more than a year, the small calculi still continued to be voided.

The very unusual disposition to form uric acid, and the complete failure of the common alkaline medicines, rendered this case particularly favourable for the trial of magnesia, as it would afford an opportunity of comparing its effects with those of the alkalies.

Previous to giving the magnesia, the urine was examined, to ascertain the quantity of uric acid it contained: this being done, the patient was directed to take fifteen grains of magnesia three times a day, in an ounce and a half of infusion of gentian: in a week the uric acid was found, by examining the urine, to have diminished in quantity, and after the first three weeks, it was only occasionally met with.

The use of magnesia has been persevered in for eight months, during which time no calculi have been voided, nor has there been any material deposit in the urine.

This patient was extremely subject to heartburn, and he likewise complained of a sense of weight and uneasiness about the region of the stomach, both of which symptoms have disappeared.

CASE II.

A gentleman, about 40 years of age, had during four years occasionally voided considerable quantities of uric acid, in the form of red sand, and had once passed a small calculus.

His urine was generally, more or less turbid, and after taking any thing which disagreed with his stomach, even in a slight degree, the red sand often made its appearance. He had never used the alkalies nor any other medicine, to alleviate his disorder, he was consequently desired to take a drachm and a half of subcarbonate of soda, dissolved in a pint and a half of water highly impregnated with carbonic acid, in the course of the day, and to persevere in this treatment for some time.

On the 30th of January 1809, he left London, and returned on the 6th of March following.

During his absence he had voided rather less uric acid than usual, but had had one severe attack, in consequence of which, twenty drops of the solution of pure potash were added to each dose of the soda water; this, however, had not the desired effect, for on the 10th of March, having taken more wine than usual on the preceding day, he was attacked with pain in the right kidney, and voided with his urine a considerable quantity of uric acid, in the form of minute red crystals. During the succeeding day, he made but little water, which deposited a copious sediment of red sand.

For the removal of this symptom, he was directed to take magnesia, in the dose of twenty grains every night and morning, in a little water; for three successive days his bowels were unusually relaxed, but afterwards became regular. He

persevered in its use for six weeks without intermission; his urine was several times examined during that period, and contained no superabundant uric acid, and he has not had the slightest return of his complaint, although he has put himself under no unusual restraint in his mode of living.

CASE III.

About the middle of October 1808, a gentleman, forty-three years of age, after taking violent horse exercise, was seized with pain in the right kidney and ureter. In the course of the night he passed a small uric calculus. For some months previous to this attack, he had felt occasional pain in the kidney, but had never voided either calculi or sand. His urine was now always turbid, and occasionally deposited red sand.

On the 28th of October he began the use of soda water, and for a time, his urine was much improved in appearance, but the uric acid gradually returned, and at the end of December, notwithstanding the continued use of the soda water, he voided more sand, and his urine was more loaded with mucus, than it had ever been before.

In consequence of these symptoms, on the 3d of January 1809, he was directed to take twenty grains of magnesia every night.

The urine was examined after the third dose, and the deposit of red sand was diminished in quantity, but it did not disappear entirely, after the magnesia had been taken for three weeks.

About this time (on the 26th of January) he caught cold, and his urine was again very turbid, but this was found to be wholly the effect of mucus, and the symptom soon left him.

On the 30th of January, he took twenty grains of magnesia, and repeated it every night and morning, until the 1st of March, when his urine was perfectly healthy, and he left it off.

On the 1st of June, he again voided a little uric acid, in the form of red crystalline sand: this attack was attended with a slight pain along the right ureter. He returned to the use of the magnesia, which he took twice a day for three weeks, in the same dose as before, and from that time to the middle of November, there had been no symptoms of a return of the complaint.

CASE IV.

A gentleman, aged fifty six, after recovering from a severe fit of the gout, voided constantly a large quantity of mucus in his urine, a symptom which he had never before noticed. There was also, occasionally, abundance of red sand, consisting principally of uric acid, but he had never voided a calculus.

His stomach was uncommonly weak, he was often affected with heartburn, and an almost constant pain in the neighbourhood of the right kidney. He had been in the habit of taking tincture of bark, and other spirituous medicines, from a belief, that the pain in his right side arose from gout in the stomach.

He had already attempted to use the alkalies, which had produced such unpleasant sensations in the stomach, that he could not be prevailed upon to try them again in any form.

Under these circumstances, he readily acceded to a new plan of treatment. He was directed to omit the use of spirituous medicines, and take twenty grains of magnesia, three

times a day in water, but this operating too powerfully upon the bowels, the same quantity of magnesia was taken twice a day only, with an addition of five drops of laudanum to each dose.

This plan was pursued without intermission for three weeks, and he received considerable benefit, as far as concerned the state of the stomach, and pain in the region of the kidney. The urine, which was examined once a week, was also, on the whole improved, but it occasionally deposited a very copious sediment, consisting of uric acid, with a variable proportion of mucous secretion.

After a further continuance of the use of the magnesia for three weeks, the urine was often much loaded with uric acid, and mucus, but these appearances, which before the use of the magnesia were constant, are now only occasional, so that the disposition to form a redundant quantity of uric acid, is much diminished: it is also deserving of remark, that there has not been the slightest symptom of gout from the time of the last attack, which is more than a year back, a longer interval of ease, than this patient has experienced for the last six years.

He has now omitted the regular use of the magnesia, but on perceiving any unpleasant sensation in the stomach, he returns to it for a week or ten days, and then again leaves it off.

From the preceding cases it appears, that the effects of magnesia taken into the stomach, are in many respects different from those produced by the alkalies, in those patients in whom there is a disposition to form a superabundant quantity of uric acid.

With a view to ascertain their comparative effects on healthy urine, when taken under the same circumstances, the following experiments were made.

Experiment 1. *On Soda.*

Two drachms of subcarbonate of soda were taken on an empty stomach at nine o'clock in the morning, dissolved in three ounces of water, and immediately afterwards, a large cup of warm tea.

In six minutes, about one ounce of urine was voided; in twenty minutes six ounces more; and after two hours, a similar quantity.

The first portion became very turbid, within ten minutes after it had been voided, and deposited a copious sediment of the phosphates, in consequence of the action of the alkali upon the urine. It slightly restored the blue colour to litmus paper reddened with vinegar: the alkali therefore, was not merely in sufficient quantity to saturate the uncombined acid in the urine, and consequently to throw down the phosphates, but it was in excess, and the urine was voided alkaline.

The urine voided after twenty minutes, also deposited a cloud of the phosphates, but the transparency of that voided two hours after the alkali had been taken, was not disturbed.

Here, therefore, the effect of the alkali upon the urine, was at its maximum, probably in less than a quarter of an hour after it had been taken into the stomach, and in less than two hours, the whole of the alkali had passed off.

Experiment 2. *On Soda, with excess of Carbonic Acid.*

The same quantity of soda, dissolved in eight ounces of

water, very highly impregnated with carbonic acid, was taken under the same circumstances as in the former experiment, and the urine was voided at nearly similar intervals.

The separation of the phosphates was less distinct, and less rapid. In two hours after the urine had been voided, there was a small deposit, composed principally of phosphate of lime; there was also a distinct pellicle on the surface, consisting of the triple phosphate of ammonia and magnesia. This appearance, produced by the escape of the carbonic acid, which had before retained the ammoniaco-magnesian phosphate in solution, and which now occasions its deposition on the surface, is by no means uncommon, even in the urine of healthy persons: in the present instance, it appears to prove, that carbonic acid passes off from the stomach, by the kidnies, for after taking the alkalies in water, very highly impregnated with it, the pellicle is uniformly produced, and is also much more abundant and distinct, than under any other circumstances.

In similar experiments with potash, the results were in all cases as similar as could be expected in researches of this nature.

Experiment 8. *On Magnesia.*

Magnesia was taken under circumstances similar to those of the soda in the former experiment: in the quantity of half a drachm, it produced no sensible effect upon the urine during the whole day. When taken in the dose of a drachm at nine o'clock in the morning, the urine voided at twelve o'clock became slightly turbid: at three o'clock the effect of the magnesia was at its maximum, and a distinct separation of the phosphates took place, partly in the form of a film, which when examined,

was found to be the triple phosphate of ammonia and magnesia, and partly in the state of a white powder, consisting almost entirely of the triple phosphate and phosphate of lime.

The effect of large doses of magnesia, in producing a white sediment in the urine, is very commonly known, and has been erroneously attributed to the magnesia, passing off by the kidnies.

These experiments, shew that magnesia, even in very large doses, neither produces so rapid an effect upon the urine, nor so copious a separation of the phosphates, as the alkalies; on this its value as a remedy in calculous disorders seems materially to depend.

Experiment 4. On Lime.

Two ounces of lime water, taken in the morning upon an empty stomach, with a cup of milk and water, produced no effect whatever.

A pint of lime water, taken at four intervals of an hour each, produced a slight deposition of the phosphates at the end of the fifth hour. The urine voided at the third hour was not at all affected; at the fifth hour, the effect appeared at its height, but was not nearly so distinct as from small doses of soda, notwithstanding the insoluble compounds which lime might be expected to form with the acids in the urine.

The unpleasant taste of lime water, the quantity in which it requires to be taken, on account of the small proportion of the earth which is held in solution, and the uncertainty of its effect, are circumstances which render it of little use, excepting in some very rare cases, where it has been found to agree particularly well with the stomach.

The effect of carbonate of lime upon the urine, was much less distinct than that of lime water: at times it produced no effect, but when taken in very large doses, a slight deposition of the phosphates was produced.

These experiments were repeated upon three different individuals, and there was always an uniformity in the results.

When the medicines were taken some hours after food being received into the stomach, their effects upon the urine were retarded, but not prevented.

The effects of many other substances upon the urine were examined into during this investigation, but they varied so much according to circumstances, that no satisfactory results were produced.

As it is found in the foregoing experiments, that the effects of soda on the urine are modified by the presence of carbonic acid, the following experiment was made, to ascertain, whether any sensible effects are produced by that acid on healthy urine.

Twelve ounces of water very highly impregnated with carbonic acid, were taken upon an empty stomach at nine o'clock in the morning. At ten o'clock about eight ounces of urine were voided, which had a natural appearance, but when compared with urine voided under common circumstances, was found to contain a superabundant quantity of carbonic acid: this gas was copiously given off when the urine was gently heated, or when it was exposed under the exhausted receiver of an air pump.

In a patient who had a calculus of large dimensions extracted from the bladder, composed entirely of the phosphates, and whose stomach did not admit of the use of stronger

acids, carbonic acid was given in water; it was found peculiarly grateful to the stomach, and upon examining the urine during its use, the phosphates were only voided in solution, but when at any time it was left off, they were voided in the form of white sand.

From the Press of
W. BULMER & Co.
Cleveland-Row, St. James's.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

MDCCCX.

METEOROLOGICAL JOURNAL

for January, 1809.

1809	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°					Inches.		Inches.			
Jan. 1	37	8	0	39	48	29,61	74	0,085	E	1	Rain.
	39	2	0	39	51	29,59	74		E	1	Cloudy.
2	38	8	0	39	47	29,48	75	0,290	E	1	Foggy.
	41	2	0	41	50	29,44	73		E	1	Cloudy.
3	31	8	0	31	47	29,32	75	0,085	NE	2	Snow.
	33	2	0	33	50	29,36	72		NNE	1	Sleet.
4	29	8	0	31	47	29,66	75		NE	1	Cloudy.
	33	2	0	33	49	29,76	73		E	1	Rain.
5	32	8	0	33	47	29,70	73	0,062	E	2	Cloudy.
	33	2	0	33	49	29,65	71		E	1	Cloudy.
6	33	8	0	39	47	29,53	73		ESE	1	Cloudy.
	47	2	0	46	50	29,30	74		S	1	Cloudy.
7	43	8	0	45	48	29,33	73		SE	2	Cloudy.
	46	2	0	45	50	29,13	72		S	2	Rain.
8	40	8	0	44	49	28,92	73	0,145	SE	2	Rain.
	47	2	0	47	52	28,50	74		SE	1	Cloudy.
9	38	8	0	40	48	29,40	73	0,235	SW	1	Cloudy.
	45	2	0	45	51	29,26	74		S	1	Rain.
10	38	8	0	42	48	29,21	75	0,403	S	1	Cloudy.
	47	2	0	47	52	29,16	73		SW	1	Cloudy.
11	36	8	0	39	49	29,38	72		SW	1	Cloudy.
	46	2	0	46	53	29,42	65		SSW	1	Fair.
12	38	8	0	39	50	29,47	72		SW	1	Cloudy.
	41	2	0	41	52	29,56	70		NW	1	Cloudy.
13	34	8	0	35	50	29,71	70		NW	1	Cloudy.
	40	2	0	39	53	29,71	70		S	1	Rain.
14	30	8	0	31	49	29,81	70	0,085	NW	1	Cloudy.
	34	2	0	34	51	29,84	63		NNE	1	Cloudy.
15	28	8	0	28	48	29,76	70		E	2	Snow.
	30	2	0	28	51	29,77	65		E	1	Cloudy.
16	27	8	0	28	46	30,05	65		NE	1	Cloudy.
	30	2	0	30	49	30,12	61		NE	1	Cloudy.

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1809	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Sc.	
Jan. 17	° 27	8	0	28	46	30,09	° 63		ENE	1	Cloudy.
	30	2	0	29	51	30,04	61		E	1	Fair.
18	30	8	0	30	44	29.92	63		E	1	Fair.
	27	2	0	27	47	29.86	63		E	1	Fair.
19	20	8	0	22	42	29.74	60		E	1	Cloudy.
	30	2	0	28	46	29.59	65		NE	1	Sleet.
20	28	8	0	31	43	29.51	67		N	1	Cloudy.
	33	2	0	33	47	29.47	70		NE	1	Cloudy.
21	32	8	0	32	44	29.48	72		N	1	Cloudy.
	35	2	0	35	48	29.54	69		NE	1	Snow.
22	31	8	0	32	44	29.16	73		NE	1	Snow.
	35	2	0	35	47	29.03	74		W	1	Cloudy.
23	22	8	0	22	43	29.65	66		SW	1	Cloudy.
	31	2	0	30	47	29.72	65		ENE	1	Sleet.
24	30	8	0	33	43	29.37	75		E	1	Rain.
	47	2	0	35	47	29.46	70		WSW	2	Cloudy.
25	30	8	0	44	46	29.50	70	1,403	NW	1	Cloudy.
	44	2	0	37	48	29.75	66		S	2	Cloudy. [much wind last night.
26	35	8	0	48	47	29.25	76	0,208	W	2	Cloudy.
	51	2	0	48	51	29.19	60		S	2	Cloudy.
27	42	8	0	45	49	29.53	73		S	1	Cloudy.
	52	2	0	52	53	29.45	73		S	2	Cloudy.
28	49	8	0	50	51	29.60	73		S	2	Fair.
	55	2	0	55	55	29.48	59		SSE	2	Cloudy.
29	47	8	0	48	51	29.30	73		S	3	Cloudy.
	52	2	0	52	55	28.95	63		S	2	Cloudy.
30	44	8	0	44	52	29.43	68		S	2	Rain.
	50	2	0	50	54	28,92	70		SW	1	Cloudy.
31	39	8	0	39	50	29.72	66	0,231	SW	1	Fine.
	47	2	0	47	55	29,88	61				

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for February, 1809.

1809	Sir's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Feb. 1	42	7	0	48	52	29,80	70		S	2	Cloudy.
	54	2	0	53	56	29,66	65		S	2	Cloudy.
2	48	7	0	49	53	29,52	68		SSW	2	Cloudy.
	52	2	0	52	56	29,51	64		SSW	2	Cloudy.
3	49	7	0	51	56	29,22	72	0,305	SSW	2	Rain.
	55	2	0	55	58	29,20	65		SW	2	Cloudy.
4	45	7	0	46	55	29,47	68	0,042	S	2	Rain.
	50	2	0	50	57	29,47	61		S	2	Cloudy.
5	43	7	0	45	54	29,40	70	0,085	S	2	Rain.
	50	2	0	50	58	29,37	67		SW	2	Cloudy.
6	44	7	0	46	54	29,36	68	0,265	SSW	1	Fine.
	51	2	0	50	57	29,40	66		SSW	2	Rain.
7	39	7	0	40	54	29,87	65	0,183	NNE	1	Cloudy.
	41	2	0	41	56	29,95	63		NE	1	Cloudy.
8	34	7	0	34	52	29,94	63		E	2	Cloudy.
	35	2	0	35	53	29,82	65		E	2	Cloudy.
9	34	7	0	42	51	29,43	72	0,225	E	2	Rain.
	53	2	0	52	54	29,34	73		SSE	1	Cloudy.
10	48	7	0	48	53	29,30	70		S	2	Cloudy.
	52	2	0	52	57	29,26	65		SSW	2	Fair.
11	46	7	0	46	54	29,13	72	0,053	SSW	1	Cloudy.
	51	2	0	50	56	29,02	62		SSW	2	Cloudy.
12	43	7	0	47	53	28,90	67	0,085	SSW	2	Cloudy.
	51	2	0	50	57	28,77	67		SSW	2	Cloudy.
13	43	7	0	46	53	28,76	64	0,052	SSW	2	Cloudy [much wind last night.
	53	2	0	52	56	28,96	58		SW	2	Fair.
14	44	7	0	44	54	29,22	70	0,070	SSW	2	Cloudy.
	52	2	0	51	57	29,30	61		SSW	2	Cloudy.
15	44	7	0	44	54	29,71	69	0,045	S	2	Cloudy.
	53	2	0	51	57	29,57	71		S	2	Cloudy.
16	43	7	0	44	54	29,82	71	0,120	S	2	Fair.
	51	2	0	50	57	29,75	63		S	2	Cloudy.

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1809	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°						°				
Feb. 17	47	7	0	47	56	29,54	70		SSW	1	Cloudy.
	53	2	0	52	57	29,72	57		SW	1	Fair.
18	46	7	0	53	57	29,79	73	0,033	SSW	2	Cloudy.
	57	2	0	57	59	29,90	60		W	2	Cloudy.
19	39	7	0	39	56	30,46	66		WSW	1	Fine.
	52	2	0	51	58	30,44	60		SW	1	Fine.
20	46	7	0	46	56	30,22	70		SSW	2	Cloudy.
	52	2	0	51	58	30,03	67		S	2	Cloudy.
21	40	7	0	40	56	29,98	58		WSW	2	Cloudy.
	43	2	0	42	56	30,10	54		NW	2	Fair.
22	34	7	0	34	54	30,32	60		WNW	1	Cloudy.
	44	2	0	44	56	30,29	56		NW	1	Cloudy.
23	40	7	0	45	54	30,10	60		NW	2	Cloudy.
	52	2	0	52	57	30,14	57		NW	2	Fair.
24	41	7	0	44	55	30,32	61		W	1	Cloudy.
	47	2	0	47	57	30,32	57		SSW	1	Fair.
25	41	7	0	41	54	30,27	61		SW	1	Cloudy.
	48	2	0	48	57	30,30	58		SW	1	Cloudy.
26	34	7	0	34	53	30,45	66		SW	1	Fair.
	47	2	0	47	57	30,42	58		SW	1	Fine.
27	38	7	0	38	53	30,38	65		WSW	1	Fair.
	51	2	0	49	56	30,37	62		SW	1	Fine.
28	35	7	0	35	53	30,34	68		SW	1	Foggy.
	53	2	0	53	57	30,30	56		WSW	1	Fine.

METEOROLOGICAL JOURNAL
for March, 1809.

1809	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Mar. 1	42	7 0	42	54	30,28	70		SW	1	Cloudy.
	50	2 0	50	56	30,25	67		E	1	Cloudy.
2	40	7 0	40	53	30,36	68	0,102	N	1	Cloudy.
	47	2 0	47	54	30,43	63		E	1	Cloudy.
3	34	7 0	34	53	30,39	66		NE	1	Fair.
	45	2 0	45	56	30,34	57		NE	1	Fine.
4	40	7 0	42	54	30,18	67		S	1	Cloudy.
	47	2 0	47	56	30,13	61		SSW	1	Cloudy.
5	40	7 0	40	53	30,18	66	0,073	NE	1	Cloudy.
	45	2 0	44	53	30,20	63		NE	1	Cloudy.
6	34	7 0	34	52	30,28	64		NE	1	Cloudy.
	41	2 0	41	54	30,31	64		NE	1	Cloudy.
7	36	7 0	36	52	30,37	66		E	1	Cloudy.
	40	2 0	40	55	30,40	64		NE	1	Cloudy.
8	36	7 0	36	52	30,47	70		E	1	Foggy.
	45	2 0	43	54	30,47	66		SW	1	Fair.
9	35	7 0	35	51	30,35	68		SW	1	Cloudy.
	53	2 0	53	54	30,28	65		W	1	Cloudy.
10	44	7 0	44	53	30,24	67		NE	1	Cloudy.
	49	2 0	48	57	30,23	55		NE	1	Fine.
11	33	7 0	33	53	30,28	63		NE	1	Fair.
	46	2 0	45	55	30,24	56		NE	1	Fair.
12	40	7 0	40	53	30,07	65		NE	2	Cloudy.
	48	2 0	48	57	30,11	56		NE	2	Fair.
13	35	7 0	36	53	30,21	63		NE	1	Cloudy.
	47	2 0	47	55	30,24	54		NE	1	Cloudy.
14	36	7 0	41	53	30,24	70		NE	1	Cloudy.
	46	2 0	46	55	30,30	60		NE	1	Fair.
15	37	7 0	37	53	30,44	61		NE	1	Cloudy.
	47	2 0	44	56	30,44	58		NE	1	Fair.
16	37	7 0	38	53	30,27	64		W	1	Cloudy.
	52	2 0	52	55	30,20	56		WNW	1	Cloudy.

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1809	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Mar. 17	2	7	0	45	54	30,20	63		NW	1	Cloudy.
	44	2	0	54	57	30,19	57		NW	1	Cloudy.
	55	7	0	45	56	30,16	61		NE	1	Cloudy.
18	45	2	0	53	57	30,11	53		NE	1	Fair.
	54	7	0	40	55	30,05	63		NE	1	Cloudy.
19	40	2	0	51	57	30,00	60		E	1	Cloudy.
	51	7	0	44	55	29,96	66		N	1	Cloudy.
20	44	2	0	50	57	30,02	63		NE	1	Cloudy.
	51	7	0	42	56	30,14	66		NE	1	Cloudy.
21	41	2	0	48	57	30,16	65		ENE	1	Cloudy.
	50	7	0	45	56	30,10	66		E	1	Cloudy.
22	43	2	0	58	58	30,04	65		S	1	Fair.
	59	7	0	45	57	29,97	63		E	1	Cloudy.
23	44	2	0	61	60	29,86	53		SSE	1	Fine.
	62	7	0	48	58	29,71	68	0,032	SW	1	Cloudy.
24	48	2	0	56	61	29,65	56		SW	1	Cloudy.
	57	7	0	48	57	29,34	68	0,158	SW	1	Rain.
25	48	2	0	51	59	29,31	56		WNW	1	Fair.
	52	7	0	42	57	29,18	63		E	1	Cloudy.
26	39	2	0	52	56	29,18	57		SE	2	Cloudy.
	53	7	0	37	56	29,29	66		ENE	1	Foggy.
27	36	2	0	56	57	29,35	55		S	1	Cloudy.
	56	7	0	43	56	29,45	65		NE	1	Foggy.
28	40	2	0	53	58	29,47	60		E	1	Cloudy.
	54	7	0	41	56	29,70	65	0,370	NE	1	Cloudy.
29	40	2	0	47	57	29,80	58		NE	1	Cloudy.
	47	7	0	38	55	29,86	63		N	1	Cloudy.
30	36	2	0	46	56	29,83	60		N	1	Cloudy
	46	7	0	44	55	29,81	64		ENE	2	Cloudy.
31	41	2	0	48	57	29,75	58		NE	2	Cloudy.
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1809	Six'o Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
Apr. 1	° 37	7 0	38	53	29,74	59		NE	2	Cloudy.
	45	2 0	45	55	29,72	60		NE	2	Cloudy.
2	32	7 0	34	53	29,82	63		NE	1	Fair.
	44	2 0	44	55	29,82	55		N	1	Hail.
3	30	7 0	32	51	29,90	60		N	1	Fine.
	44	2 0	41	54	29,92	54		NE	2	Cloudy.
4	29	7 0	31	51	30,05	63		NE	2	Cloudy.
	41	2 0	40	52	30,12	59		N	2	Snow.
5	29	7 0	32	50	30,28	60		NW	1	Cloudy.
	42	2 0	42	51	30,30	56		NNE	1	Fair.
6	30	7 0	33	50	30,28	63		NW	1	Hazy.
	48	2 0	46	52	30,17	54		SW	1	Cloudy.
7	40	7 0	40	50	30,14	70	0,150	E	1	Rain.
	47	2 0	46	54	30,22	60		E	1	Cloudy.
8	36	7 0	38	51	30,32	60		E	1	Fair.
	51	2 0	51	54	30,32	52		S	1	Fair.
9	38	7 0	41	51	30,16	62		SSW	1	Cloudy.
	54	2 0	53	53	30,05	57		S	2	Cloudy.
10	43	7 0	44	52	29,92	68		NW	1	Cloudy.
	57	2 0	56	56	29,78	57		WNW	1	Hazy.
11	47	7 0	47	53	29,56	64		NW	1	Cloudy.
	55	2 0	54	56	29,40	63		W	1	Rain.
12	33	7 0	35	53	29,80	60	0,105	NW	1	Fair.
	51	2 0	50	55	29,81	50		NW	1	Fair.
13	42	7 0	44	53	29,36	66	0,043	W	1	Cloudy.
	54	2 0	53	56	29,21	55		SW	2	Cloudy.
14	40	7 0	42	53	29,08	64	0,147	SW	2	Fine.
	52	2 0	48	55	29,06	63		SSW	1	Rain. [Lightning & thunder.
15	42	7 0	43	54	29,40	65	0,305	NW	1	Fair.
	50	2 0	50	55	29,59	56		NW	1	Cloudy.
16	42	7 0	46	53	29,07	70	0,200	S	2	Rain.
	56	2 0	55	57	29,06	56		S	2	Cloudy.

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1809	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°						°				
Apr. 17	40	7	0	40	54	29,08	70	0,570	W	1	Rain.
	45	2	0	44	55	29,24	65		NW	1	Rain.
18	34	7	0	35	52	29,55	67	0,215	NE	1	Sleet.
	45	2	0	45	53	29,62	60		NNE	2	Cloudy.
19	30	7	0	33	51	29,80	63		NE	2	Fine.
	47	2	0	47	53	29,75	52		NE	1	Cloudy.
20	35	7	0	35	51	29,75	64	0,045	NE	1	Snow.
	46	2	0	46	53	29,68	60		SW	1	Cloudy.
21	34	7	0	36	52	29,58	70	0,525	NE	1	Snow. [much snow last night.
	50	2	0	49	53	29,59	60		SE	2	Cloudy.
22	45	7	0	46	53	29,65	62	0,205	NE	2	Cloudy.
	50	2	0	48	55	29,74	65		NE	1	Cloudy.
23	43	7	0	43	53	30,00	67		NE	2	Cloudy.
	46	2	0	46	53	30,10	64		NE	2	Cloudy.
24	42	7	0	42	52	30,29	70		NE	2	Cloudy.
	49	2	0	47	54	30,32	65		NE	2	Cloudy.
25	38	7	0	40	53	30,34	67		NE	2	Cloudy.
	50	2	0	50	55	30,24	63		NE	1	Hazy.
26	47	7	0	47	54	29,93	70	0,035	SW	1	Rain.
	50	2	0	50	55	29,80	67		SSW	1	Rain.
27	48	7	0	49	54	29,70	72	0,240	SSW	1	Cloudy.
	55	2	0	53	56	29,64	63		S	1	Cloudy.
28	48	7	0	49	56	29,40	68	0,065	SE	1	Rain.
	56	2	0	53	58	29,44	63		E	1	Rain.
29	40	7	0	41	56	29,63	65	0,125	NE	2	Cloudy.
	47	2	0	47	56	29,72	55		NE	2	Cloudy.
30	36	7	0	40	55	29,78	58		NE	1	Fine.
	54	2	0	53	57	29,68	52		N	1	Fair.

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1809	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
May 1	47	7	0	49	55	29,37	68		WSW	1	Cloudy.
	53	2	0	50	57	29,28	63		SW	1	Rain.
2	38	7	0	42	54	29,43	63	0,180	WNW	1	Fair.
	53	2	0	49	57	29,52	53		NW	1	Cloudy. [Thunder.
3	36	7	0	40	55	29,80	61	0,122	W	1	Fair.
	54	2	0	53	57	29,85	48		WSW	1	Hazy.
4	40	7	0	45	55	29,96	63		W	1	Hazy.
	57	2	0	55	58	29,92	57		SW	1	Cloudy.
5	48	7	0	50	57	29,90	67	0,023	SW	1	Cloudy.
	57	2	0	55	58	30,02	52		NW	1	Fair.
6	42	7	0	46	57	30,23	58		NW	1	Cloudy.
	60	2	0	59	59	30,24	50		NW	1	Fair.
7	50	7	0	53	58	30,33	60		NW	1	Fair.
	68	2	0	67	62	30,33	48		SSW	1	Fair.
8	50	7	0	54	59	30,34	58		S	1	Fine.
	68	2	0	67	63	30,30	50		E	1	Fair.
9	47	7	0	51	59	30,23	55		NE	1	Hazy.
	66	2	0	66	65	30,12	52		E	1	Hazy.
10	47	7	0	53	61	30,08	62		E	2	Fair.
	68	2	0	66	65	30,01	55		E	1	Fair.
11	50	7	0	55	62	30,00	60		E	1	Hazy.
	72	2	0	72	65	30,00	50		E	1	Fine.
12	55	7	0	59	64	30,01	60		W	1	Fine.
	76	2	0	75	66	29,98	48		S	1	Fine.
13	51	7	0	55	64	30,00	64		SW	1	Fine.
	76	2	0	75	67	29,94	50		S	1	Fine.
14	53	7	0	59	65	29,96	56		E	1	Fine.
	75	2	0	74	67	29,94	49		E	1	Fine.
15	56	7	0	60	66	29,85	60		E	1	Hazy.
	67	2	0	64	66	29,83	60		E	1	Rain.
16	54	7	0	58	65	29,79	62		E	1	Hazy.
	73	2	0	73	67	29,78	50		E	1	Fine.

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1809	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
May. 17	55	7 0	60	66	29,88	60		ENE	1	Hazy.
	75	2 0	74	68	29,86	50		E	1	Hazy.
18	58	7 0	63	66	29,82	58		E	1	Cloudy.
	78	2 0	78	68	29,77	51		SE	1	Hazy.
19	64	7 0	64	68	29,60	59		E	1	Rain.
	72	2 0	71	69	29,55	55		ESE	2	Fair.
20	55	7 0	58	67	29,78	63	0,285	S	2	Cloudy. Mark thunder & lightning this afternoon.
	66	2 0	66	66	29,83	58		SSE	2	Cloudy.
21	51	7 0	56	64	29,95	60		S	2	Fair.
	63	2 0	62	64	29,95	56		S	2	Cloudy.
22	50	7 0	53	63	30,18	62		SSW	1	Fair.
	69	2 0	68	65	30,21	53		SE	1	Fair.
23	51	7 0	56	63	30,26	58		NE	1	Hazy.
	68	2 0	68	65	30,24	50		E	1	Fine.
24	48	7 0	52	63	30,20	62		NE	1	Fair.
	68	2 0	68	65	30,15	50		E	1	Cloudy.
25	50	7 0	51	62	30,10	62		NE	1	Cloudy.
	61	2 0	61	63	30,00	55		E	1	Cloudy.
26	51	7 0	53	61	29,90	60		E	1	Hazy.
	65	2 0	65	63	29,82	56		SSW	1	Cloudy.
27	56	7 0	58	62	29,60	65		S	2	Cloudy.
	69	2 0	69	63	29,62	50		S	1	Fine.
28	52	7 0	56	62	29,64	60		S	2	Fair.
	71	2 0	70	64	29,61	52		S	2	Fine.
29	52	7 0	56	62	29,54	60	0,120	SSW	2	Fair.
	67	2 0	67	63	29,48	53		WSW	1	Cloudy.
30	47	7 0	49	62	29,78	60	0,082	WSW	1	Fair.
	64	2 0	63	62	29,90	47		SW	2	Cloudy.
31	51	7 0	54	61	29,77	60		S	2	Cloudy.
	63	2 0	61	61	29,73	54				

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1809	Six's Therm. least and greatest Heat.	Time,		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
June 1	54	7	0	58	60	29,50	58		SE	2	Cloudy.
	74	2	0	73	64	29,40	51		SSE	2	Fair.
2	51	7	0	53	61	29,33	55		S	2	Cloudy. [much wind last night.
	60	2	0	58	61	29,56	54		S	2	Cloudy.
3	45	7	0	50	59	30,04	57		S	2	Fine.
	64	2	0	62	61	30,00	48		SE	2	Fair.
4	51	7	0	56	61	29,68	60	0,048	ESE	1	Cloudy.
	67	2	0	67	63	29,67	53		S	2	Fair.
5	53	7	0	58	61	29,37	65		E	2	Cloudy.
	64	2	0	63	62	29,30	58		ESE	2	Cloudy.
6	52	7	0	55	60	29,58	57	0,154	SSE	2	Fair.
	62	2	0	58	61	29,64	60		S	2	Rain.
7	50	7	0	54	59	29,71	63	0,072	S	2	Rain.
	64	2	0	64	61	29,79	53		S	2	Fair.
8	50	7	0	54	59	29,85	62	0,088	S	2	Cloudy.
	61	2	0	60	60	29,67	62		S	2	Rain.
9	51	7	0	53	59	29,55	62	0,090	SSW	1	Cloudy.
	62	2	0	58	60	29,57	57		W	1	Cloudy.
10	48	7	0	52	58	29,55	63	0,020	SW	1	Cloudy.
	63	2	0	62	60	29,55	56		SW	1	Fair.
11	51	7	0	51	58	29,69	53	0,123	W	1	Cloudy.
	60	2	0	58	60	29,82	57		NW	1	Cloudy.
12	50	7	0	54	59	30,09	63		SW	1	Fine.
	70	2	0	69	61	30,09	55		SW	1	Fair.
13	54	7	0	57	59	30,00	66		SW	1	Cloudy.
	71	2	0	70	61	30,01	52		SW	1	Fair.
14	55	7	0	59	60	29,97	64		SW	1	Cloudy.
	69	2	0	68	62	29,91	55		S	2	Fair.
15	55	7	0	57	60	29,88	61		SSW	1	Cloudy.
	67	2	0	66	62	29,88	50		N	1	Hazy.
16	51	7	0	55	61	29,96	54		NNE	1	Hazy.
	70	2	0	70	63	29,94	48		NW	1	Fair.

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1809	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
June 17	53	7	0	55	61	29,85	61		SW	2	Cloudy.
	74	2	0	73	63	29,77	49		WSW	2	Hazy.
18	53	7	0	55	61	29,78	58		NW	2	Fine.
	65	2	0	63	62	29,84	50		NW	2	Fair.
19	48	7	0	54	61	29,90	58		NW	1	Fair.
	69	2	0	69	63	29,90	48		NE	1	Fair.
20	57	7	0	60	62	29,99	64		NNE	1	Cloudy.
	77	2	0	75	64	30,06	52		NNE	1	Fair.
21	56	7	0	60	64	30,21	60		NNE	1	Fine.
	76	2	0	74	66	30,24	51		E	1	Fine.
22	56	7	0	60	65	30,32	60		E	1	Cloudy.
	73	2	0	70	66	30,31	54		E	1	Cloudy.
23	59	7	0	62	65	30,31	63		NE	1	Fair.
	78	2	0	78	67	30,27	50		NE	1	Hazy.
24	56	7	0	61	66	30,34	59		NE	1	Fair.
	78	2	0	77	68	30,33	48		ENE	1	Fine.
25	56	7	0	58	67	30,44	58		NE	1	Fair.
	70	2	0	69	67	30,43	50		NNE	1	Fine.
26	47	7	0	52	65	30,42	58		NE	2	Fair.
	67	2	0	66	66	30,35	49		NE	2	Fair.
27	48	7	0	52	64	30,24	60		NE	1	Cloudy.
	70	2	0	70	66	30,10	46		NE	1	Fair.
28	55	7	0	55	65	30,08	65		NE	1	Rain.
	67	2	0	64	65	30,03	56		NE	1	Cloudy.
29	53	7	0	53	64	30,02	64	0,410	NE	1	Cloudy.
	63	2	0	61	64	29,99	60		NE	1	Cloudy.
30	50	7	0	55	63	29,95	63		SNE	1	Cloudy.
	70	2	0	70	65	29,88	51		SW	1	Fair.

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1809	Six's Therm least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
July 1	54	7 0	59	63	29,81	64		S	2	Cloudy.
	71	2 0	70	65	29,80	53		S	2	Fair.
2	56	7 0	61	64	29,78	61		E	1	Fair.
	73	2 0	69	65	29,72	56		W	1	Cloudy.
3	52	7 0	54	63	29,54	61	0,410	WSW	1	Fair.
	62	2 0	57	64	29,51	55		NW	1	Cloudy.
4	50	7 0	50	62	29,45	64	0,200	SW	1	Rain.
	60	2 0	60	62	29,44	60		S	2	Rain. [Thunder & lightning.
5	51	7 0	54	62	29,55	66	0,340	NE	1	Hazy.
	63	2 0	59	62	29,58	62		ENE	1	Rain.
6	53	7 0	55	61	29,71	66	0,052	E	1	Rain.
	60	2 0	63	62	29,72	67		E	1	Rain.
7	58	7 0	58	62	29,83	73	0,295	NE	1	Rain.
	69	2 0	69	62	29,81	63		NE	1	Fair. { Much lightning & thunder last night.
8	59	7 0	61	62	29,80	80	0,283	NE	1	Rain. { Much lightning & thunder last night.
	72	2 0	71	65	29,85	53		E	1	Fair. { Much lightning & thunder last night.
9	53	7 0	54	63	29,88	70	0,318	NE	2	Rain.
	57	2 0	57	62	29,84	65		N	2	Cloudy.
10	54	7 0	54	62	29,88	71	0,075	N	2	Rain.
	61	2 0	59	62	29,92	72		NNE	1	Cloudy.
11	51	7 0	51	61	30,05	66	0,155	NE	1	Cloudy.
	68	2 0	66	63	30,05	56		NNE	1	Fine.
12	55	7 0	60	62	30,10	68		W	1	Hazy.
	74	2 0	74	64	30,08	53		W	1	Cloudy.
13	63	7 0	63	63	30,09	62		N	1	Cloudy.
	71	2 0	70	65	30,13	48		NE	1	Fine.
14	54	7 0	58	64	30,20	57		WSW	1	Fair.
	74	2 0	74	66	30,17	48		SW	1	Fair.
15	58	7 0	61	65	30,08	60		WSW	1	Cloudy.
	73	2 0	72	65	30,04	53		W	1	Cloudy.
16	58	7 0	62	65	29,99	62		W	1	Fair.
	75	2 0	74	67	29,92	47		WNW	2	Fair.

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		H. M.	°	°	Inches.		Inches.	Points.	Str.	
July 17	°					°				
	54	7 0	62	65	29.74	62		W	1	Cloudy.
	70	2 0	69	66	29.74	52		W	1	Cloudy.
18	51	7 0	52	65	29,90	56		NE	1	Cloudy.
	63	2 0	63	65	29.94	46		WNW	1	Cloudy.
19	50	7 0	56	63	30,02	55		WNW	1	Fine.
	70.	2 0	69	64	30,04	49		WNW	1	Hazy.
20	57	7 0	60	63	30,09	62		NNW	1	Cloudy.
	72	2 0	71	66	30,17	53		NE	1	Fair.
21	54	7 0	56	64	30,22	66		NNE	1	Fair.
	72	2 0	71	65	30,18	46		E	1	Cloudy.
22	53	7 0	57	64	30,06	62		NE	1	Cloudy.
	66	2 0	65	64	30,00	56		NE	1	Cloudy.
23	54	7 0	56	63	29.90	63		NE	1	Cloudy.
	74	2 0	73	66	29.84	52		E	1	Fair.
24	53	7 0	57	65	29.84	70		NE	1	Cloudy.
	71	2 0	70	67	29.81	57		NE	1	Fine.
25	58	7 0	59	66	29,77	73		NE	1	Cloudy.
	78	2 0	78	68	29.74	58		N	1	Fair.
26	60	7 0	60	67	29.74	73	0,103	N	1	Cloudy. { Last night lightning & thunder.
	74	2 0	73	68	29,73	64		N	1	Cloudy.
27	61	7 0	61	68	29.72	74	0,135	N	1	Cloudy. { Last night lightning & thunder.
	73	2 0	72	68	29.72	62		SW	1	Cloudy.
28	61	7 0	61	68	29,63	63	0,052	SW	1	Cloudy.
	69	2 0	67	67	29.64	64		W	1	Cloudy.
29	54	7 0	59	65	29.75	60	0,035	SW	2	Fair.
	71	2 0	69	67	29,76	50		SSW	2	Cloudy.
30	59	7 0	59	65	29,60	72	0,195	SSW	1	Cloudy.
	71	2 0	69	66	29,58	56		S	1	Cloudy.
31	56	7 0	60	64	29,54	65	0,115	S	2	Cloudy.
	69	2 0	69	66	29,53	53		S	2	Cloudy.

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		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Aug. 1	57	7 0	58	64	29,64	64	0,042	SSW	2	Fair.
	72	2 0	71	66	29,68	53		NW	1	Fair.
2	58	7 0	58	64	29,74	63		SSW	1	Fair.
	71	2 0	70	65	29,70	51		SW	2	Fair.
3	57	7 0	59	64	29,38	67		SSE	2	Rain.
	69	2 0	69	66	29,34	53		SSW	2	Cloudy.
4	56	7 0	54	63	29,42	60	0,062	SSW	2	Fair.
	61	2 0	59	63	29,52	66		W	2	Rain.
5	50	7 0	54	63	29,78	62	0,055	SW	1	Fair.
	65	2 0	62	63	29,74	60		S	2	Rain.
6	57	7 0	60	63	29,40	72	0,048	S	2	Cloudy.
	68	2 0	68	64	29,30	58		S	2	Cloudy.
7	56	7 0	56	63	29,60	62	0,065	WSW	2	Cloudy.
	68	2 0	66	63	29,76	53		W	2	Cloudy.
8	55	7 0	60	63	29,93	71		WSW	1	Cloudy.
	73	2 0	72	65	29,96	54		SW	2	Fair.
9	56	7 0	60	64	29,95	67		E	1	Cloudy.
	74	2 0	74	66	29,90	53		SE	2	Fair.
10	59	7 0	62	65	29,90	65		E	1	Hazy.
	79	2 0	79	68	29,82	50		ESE	1	Fair.
11	65	7 0	65	67	29,69	67	0,160	SW	1	Cloudy. Much lightning & thunder last night.
	78	2 0	77	69	29,71	52		SSW	2	Fair.
12	57	7 0	59	66	29,69	65	0,030	S	1	Cloudy.
	66	2 0	63	66	29,68	59		S	1	Cloudy.
13	55	7 0	58	64	29,80	65	0,210	SSW	2	Cloudy.
	66	2 0	65	65	29,80	58		S	2	Cloudy.
14	55	7 0	58	64	29,89	66	0,095	S	2	Cloudy.
	68	2 0	67	65	29,78	56		S	2	Cloudy.
15	59	7 0	60	63	29,75	71	0,065	SW	1	Cloudy.
	70	2 0	68	64	29,78	57		SSW	1	Cloudy.
16	60	7 0	61	63	29,87	74	0,045	S	1	Cloudy
	72	2 0	71	65	29,93	60		S	2	Cloudy.

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		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Aug. 17	59	7 0	62	64	29,85	67	0,072	E	1	Cloudy.
	79	2 0	78	68	29,80	55		S	1	Fair.
18	58	7 0	59	65	29,81	65	0,045	S	1	Rain.
	71	2 0	70	67	29,75	60		S	2	Fair.
19	57	7 0	59	64	29,82	70	0,063	SSE	2	Fine.
	69	2 0	69	66	29,76	55		S	2	Cloudy.
20	57	7 0	58	65	29,93	67	0,042	SSW	1	Cloudy.
	68	2 0	68	66	29,96	56		S	2	Fair.
21	56	7 0	58	64	29,85	70		S	2	Cloudy.
	65	2 0	64	64	29,78	68		S	2	Rain.
22	52	7 0	55	63	29,84	65	0,075	S	1	Fine.
	68	2 0	67	65	29,78	52		SSW	2	Fair.
23	52	7 0	55	63	29,60	66	0,030	S	1	Fine.
	68	2 0	68	65	29,54	51		SSW	2	Fair.
24	50	7 0	53	63	29,48	65		S	1	Fine.
	66	2 0	63	64	29,43	56		S	2	Fair.
25	49	7 0	51	62	29,42	56	0,190	SW	1	Fair.
	65	2 0	61	63	29,48	57		S	1	Cloudy.
26	49	7 0	52	62	29,83	67	0,033	SW	1	Fair.
	65	2 0	60	62	29,78	63		S	2	Cloudy.
27	53	7 0	55	62	29,76	64		SW	1	Cloudy.
	65	2 0	65	62	29,78	58		W	1	Cloudy.
28	54	7 0	55	62	29,96	70		SW	1	Fair.
	70	2 0	70	64	30,05	50		SSW	1	Fair.
29	56	7 0	58	62	30,09	65		SSW	1	Cloudy.
	72	2 0	72	65	30,07	49		S	1	Fine.
30	61	7 0	62	65	29,88	63		E	1	Fair.
	77	2 0	77	67	29,85	56		S	1	Fair.
31	63	7 0	63	65	29,85	64		NE	1	Cloudy.
	65	2 0	65	66	29,84	64		NE	1	Cloudy.

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1809	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Sept. 1	58	7	0	58	65	29,80	66		NE	1	Cloudy.
	64	2	0	64	65	29,79	63		NE	1	Cloudy.
2	59	7	0	60	64	29,67	65		ENE	1	Cloudy.
	72	2	0	72	66	29,62	57		E	1	Fair.
3	60	7	0	61	66	29,60	71		E	1	Fine.
	71	2	0	71	67	29,63	56		SSE	2	Cloudy.
4	60	7	0	61	66	29,61	68	0,032	NE	1	Cloudy.
	70	2	0	64	67	29,58	73		NE	1	Rain. [Thunder.
5	59	7	0	59	66	29,53	77	0,750	N	1	Foggy.
	72	2	0	71	67	29,50	60		SSW	1	Fair.
6	59	7	0	59	67	29,52	73		SW	1	Cloudy.
	71	2	0	71	68	29,49	56		S	1	Fair.
7	59	7	0	59	66	29,26	71	0,075	E	1	Cloudy.
	66	2	0	65	67	29,28	60		S	2	Fair.
8	54	7	0	55	65	29,31	68	0,050	NE	1	Rain.
	60	2	0	65	66	29,44	57		N	1	Cloudy.
9	53	7	0	54	64	29,58	71	0,075	NW	1	Cloudy.
	67	2	0	67	66	29,66	55		NW	1	Fair.
10	54	7	0	55	65	29,69	67		WSW	1	Fine.
	67	2	0	66	65	29,67	56		SW	1	Cloudy.
11	51	7	0	52	64	29,68	70		WSW	1	Cloudy.
	65	2	0	65	66	29,72	64		WNW	1	Fair.
12	50	7	0	52	63	29,80	66	0,056	W	1	Cloudy.
	65	2	0	65	64	29,81	53		NW	1	Fair.
13	48	7	0	50	63	29,83	66		SW	1	Fine.
	63	2	0	63	63	29,83	60		SW	1	Cloudy.
14	56	7	0	56	62	29,66	71	0,205	ESE	2	Cloudy.
	64	2	0	64	63	29,65	61		ESE	2	Cloudy.
15	54	7	0	55	62	29,92	74		NE	1	Fine.
	60	2	0	65	63	30,06	56		NE	1	Cloudy.
16	50	7	0	52	62	30,16	66		N	1	Cloudy.
	65	2	0	65	62	30,09	60		SW	1	Cloudy.

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	°			°	°	Inches.		Inches.			
Sept. 17	57	7	0	57	62	30,00	71		SSW	1	Cloudy.
	67	2	0	67	63	29,98	54		WSW	1	Cloudy.
18	54	7	0	56	62	29,93	69		S	1	Cloudy.
	64	2	0	64	63	29,79	68		S	2	Cloudy.
19	52	7	0	53	61	29,60	60	0,030	W	2	Fair.
	63	2	0	62	63	29,76	52		W	2	Fair.
20	49	7	0	52	61	29,55	65		E	2	Cloudy.
	66	2	0	64	61	29,32	73		S	2	Rain.
21	55	7	0	56	61	29,52	65	0,095	WSW	2	Cloudy.
	64	2	0	64	62	29,80	53		NW	2	Fair.
22	54	7	0	57	61	29,59	73	0,183	S	2	Rain.
	69	2	0	67	64	29,68	55		WSW	2	Fair.
23	59	7	0	61	62	29,63	66		SSW	2	Cloudy.
	66	2	0	66	64	29,70	54		SW	2	Fair.
24	51	7	0	52	62	29,83	62	0,110	SSW	2	Fair.
	60	2	0	55	61	29,85	63		SSW	2	Rain.
25	50	7	0	57	60	29,66	65	0,085	SSW	2	Rain.
	58	2	0	52	60	29,75	66		NW	1	Rain.
26	42	7	0	43	58	29,98	70	0,420	SW	1	Fine.
	58	2	0	58	60	30,01	52		WSW	1	Fine.
27	49	7	0	53	58	29,56	70	0,060	S	2	Rain.
	64	2	0	60	59	29,85	66		SSW	2	Rain.
28	41	7	0	42	57	29,81	64	0,180	WSW	1	Fine.
	55	2	0	47	57	29,84	60		NW	1	Rain.
29	40	7	0	40	57	29,90	66	0,090	W	1	Fair.
	53	2	0	53	59	29,98	55		W	1	Fair.
30	43	7	0	46	57	29,94	67		SW	1	Rain.
	58	2	0	57	58	29,85	72		SW	1	Rain.

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		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Oct. 1	49	7 0	50	57	30,10	67	0,030	S	1	Cloudy.
	57	2 0	57	58	30,18	57		N	1	Fair.
2	50	7 0	53	57	30,28	70		SSW	1	Cloudy.
	61	2 0	61	59	30,31	72		S	1	Cloudy.
3	57	7 0	57	58	30,32	73		SSW	1	Cloudy.
	63	2 0	62	60	30,30	67		S	1	Cloudy.
4	56	7 0	56	58	30,19	68		E	1	Cloudy.
	62	2 0	62	62	30,16	58		ESE	1	Fine.
5	47	7 0	48	59	30,08	70		NE	1	Fair.
	61	2 0	61	63	30,08	58		E	1	Fine.
6	52	7 0	53	60	30,06	76		NE	1	Cloudy.
	62	2 0	61	62	30,05	58		E	1	Cloudy.
7	50	7 0	52	60	30,05	69		NNE	1	Cloudy.
	60	2 0	59	62	30,07	57		E	1	Fine.
8	43	7 0	44	59	30,09	67		NNE	1	Fair.
	57	2 0	56	60	30,10	57		ENE	2	Fine.
9	43	7 0	44	57	30,09	62		NE	2	Fine.
	52	2 0	51	59	30,08	53		E	2	Fine.
10	40	7 0	42	56	30,04	60		NNE	1	Cloudy.
	55	2 0	50	57	30,03	52		ENE	2	Fair.
11	43	7 0	45	56	30,00	57		E	2	Fine.
	50	2 0	50	57	30,03	53		E	2	Fine.
12	40	7 0	40	55	30,12	58		NNE	1	Cloudy.
	49	2 0	49	58	30,11	54		E	1	Fair.
13	35	7 0	36	55	30,20	60		NNE	1	Fair.
	48	2 0	48	58	30,21	54		E	1	Fair.
14	35	7 0	36	54	30,28	61		NE	1	Fair.
	51	2 0	51	57	30,28	57		N	1	Fair.
15	36	7 0	37	53	30,34	65		NNE	1	Cloudy.
	49	2 0	49	56	30,28	60		S	1	Fine.
16	40	7 0	42	53	30,13	65		SSW	1	Cloudy.
	56	2 0	56	56	30,08	64		SSW	1	Cloudy.

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	°			°	°	Inches.	°	Inches.			
Oct. 17	53	7	0	53	54	30,13	75		SW	1	Cloudy.
	60	2	0	60	57	30,10	60		SSW	1	Cloudy.
18	53	7	0	53	55	30,08	75	0,083	SSW	1	Rain.
	59	2	0	59	58	30,10	70		SW	1	Cloudy.
19	55	7	0	55	57	30,14	77		SW	1	Cloudy.
	59	2	0	59	59	30,15	76		SW	1	Cloudy.
20	53	7	0	53	57	30,15	72		SW	1	Cloudy.
	55	2	0	55	58	30,17	67		SSW	1	Cloudy.
21	51	7	0	52	57	30,15	72		ESE	1	Cloudy.
	56	2	0	56	59	30,10	65		SE	1	Cloudy.
22	51	7	0	52	58	30,09	65		S	1	Cloudy.
	54	2	0	53	59	30,07	62		S	1	Cloudy.
23	50	7	0	52	58	30,00	71		SE	1	Cloudy.
	58	2	0	57	59	29,95	65		S	1	Cloudy.
24	48	7	0	50	58	29,88	73		E	1	Fine.
	61	2	0	61	61	29,85	66		SE	1	Fine.
25	51	7	0	51	59	30,10	68		SW	1	Fine.
	57	2	0	57	60	30,22	60		E	1	Fair.
26	51	7	0	52	59	30,27	74	0,025	E	1	Fair.
	65	2	0	65	63	30,26	69		E	1	Fair.
27	50	7	0	50	60	30,28	74		NE	1	Foggy.
	62	2	0	62	62	30,24	72		NE	1	Fair.
28	49	7	0	49	60	30,27	76				Foggy.
	53	2	0	53	61	30,26	75				Foggy.
29	48	7	0	48	59	30,26	77		SE	1	Foggy.
	50	2	0	50	60	30,24	73		SE	1	Foggy.
30	48	7	0	49	58	30,20	76		E	1	Foggy.
	52	2	0	52	60	30,15	75		E	1	Cloudy.
31	47	7	0	47	58	30,10	76		NE	1	Foggy.
	50	2	0	50	59	30,05	71		NE	1	Cloudy.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
Nov. 1	44	7 0	48	58	30,07	75		NNE	1	Cloudy.
	54	2 0	53	58	30,10	67		NE	1	Cloudy.
2	43	7 0	46	56	30,20	60		NNW	1	Cloudy.
	51	2 0	51	57	30,21	58		N	1	Cloudy.
3	40	7 0	40	55	30,13	67		NE	1	Fair.
	48	2 0	48	57	30,07	59		NE	1	Cloudy.
4	41	7 0	42	54	29,95	67		NE	1	Cloudy.
	48	2 0	48	56	29,80	70		N	1	Rain.
5	42	7 0	42	54	29,81	72	0,190	NW	1	Fair.
	46	2 0	45	56	29,87	71		SW	1	Rain.
6	38	7 0	41	53	29,92	73	0,128	SW	1	Cloudy.
	47	2 0	47	57	29,85	67		SW	1	Fair.
7	38	7 0	39	53	30,01	73		NE	1	Fair.
	50	2 0	49	55	30,11	70		NE	1	Cloudy.
8	41	7 0	41	53	30,38	77		NE	1	Foggy.
	48	2 0	48	55	30,41	72		NNE	1	Cloudy.
9	42	7 0	42	53	30,40	73		NE	1	Cloudy.
	50	2 0	50	55	30,40	68		NE	1	Cloudy.
10	46	7 0	47	55	30,34	67		NE	1	Cloudy.
	51	2 0	51	57	30,27	66		ENE	1	Cloudy.
11	43	7 0	43	54	30,16	65		E	1	Cloudy.
	44	2 0	43	56	30,10	65		E	1	Cloudy.
12	42	7 0	43	54	29,92	73		E	1	Cloudy.
	46	2 0	45	56	29,80	73		E	1	Cloudy.
13	43	7 0	43	53	29,73	73	0,105	SW	1	Cloudy.
	47	2 0	47	56	29,75	68		SW	1	Cloudy.
14	38	7 0	38	53	29,72	72		NNE	1	Foggy.
	48	2 0	48	56	29,64	71		NE	1	Cloudy.
15	38	7 0	38	54	29,65	70	0,145	NNE	1	Cloudy.
	40	2 0	40	55	29,67	58		N	1	Fair.
16	30	7 0	30	52	29,68	60		NW	1	Fair.
	37	2 0	37	53	29,74	57		NW	1	Fair.

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	°			°	°	Inches.	°	Inches.	Points.	Str.	
Nov. 17	32	7	0	38	52	29,56	70		WSW	1	Snow.
	47	2	0	45	53	29.44	56		NW	2	Cloudy.
18	30	7	0	30	50	29.71	62		W	1	Fair.
	38	2	0	38	52	29,80	62		NW	1	Fair.
19	29	7	0	30	48	30,23	66		NNE	1	Fine.
	38	2	0	38	52	30,35	63		N	1	Fine.
20	25	7	0	26	47	30,46	64		WSW	1	Fine.
	38	2	0	38	51	30,36	63		WSW	1	Fine.
21	34	7	0	38	48	30,28	70	0,043	N	1	Cloudy.
	42	2	0	41	51	30,31	62		NW	1	Fair.
22	34	7	0	37	48	30.18	73		WSW	1	Cloudy.
	50	2	0	50	52	30,05	63		W	1	Cloudy.
23	41	7	0	41	49	30,05	72		SW	1	Fair.
	49	2	0	49	53	29.93	67		W	1	Cloudy.
24	44	7	0	44	51	29.37	70	0,380	WSW	1	Rain.
	45	2	0	44	53	29.37	70		NW	1	Fair.
25	37	7	0	37	52	29.78	65	0,023	N	1	Fair.
	43	2	0	43	53	29.66	67		SW	1	Cloudy.
26	38	7	0	42	51	29.10	70	0,045	SW	1	Rain.
	45	2	0	45	54	29.14	64		W	1	Cloudy.
27	32	7	0	32	50	29.57	70		NNE	1	Fine.
	42	2	0	42	53	29,78	70		N	1	Fair.
28	32	7	0	32	50	29,96	73		N	1	Foggy.
	40	2	0	39	52	29.90	73		SW	1	Cloudy.
29	35	7	0	35	48	29,88	74	0,033	W	1	Foggy.
	42	2	0	42	52	29.87	72		SW	1	Cloudy.
30	33	7	0	33	48	29,90	74				Foggy.
	44	2	0	43	52	29.80	72		SW	1	Cloudy.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
Dec. 1	43	8 0	44	51	29,27	75	0,403	SSW	1	Cloudy.
	45	2 0	44	53	29,33	65		WNW	1	Fair.
2	35	8 0	35	51	29,67	70	0,050	W	1	Fair.
	45	2 0	45	54	29,80	65		NW	1	Cloudy.
3	34	8 0	37	49	29,93	68		SW	1	Cloudy.
	47	2 0	47	52	29,82	68		S	1	Cloudy.
4	44	8 0	44	50	29,51	76	0,055	W	1	Fair.
	48	2 0	48	54	29,44	70		SW	1	Cloudy.
5	38	8 0	38	50	29,86	70		WSW	1	Cloudy.
	48	2 0	48	53	30,03	65		WNW	1	Fair.
6	38	8 0	45	52	30,02	75		SW	2	Cloudy.
	51	2 0	50	53	29,97	76		SSW	2	Cloudy.
7	50	8 0	54	54	29,84	76	0,073	S	2	Cloudy.
	54	2 0	52	54	29,92	70		SW	2	Cloudy.
8	38	8 0	40	53	30,24	68	0,100	W	1	Cloudy.
	49	2 0	46	55	30,11	67		S	1	Cloudy.
9	44	8 0	46	54	29,67	76	0,055	W	1	Cloudy.
	52	2 0	48	57	29,54	67		S	2	Cloudy.
10	46	8 0	46	55	29,15	70	0,090	SSW	1	Cloudy.
	50	2 0	50	57	29,17	63		SSW	1	Fair.
11	38	8 0	38	53	29,34	68	0,205	SW	2	Fair.
	43	2 0	43	56	29,47	65		SW	1	Fair.
12	40	8 0	46	52	28,97	73	0,212	S	2	Rain [much wind last night.
	48	2 0	45	55	29,05	64		SW	2	Cloudy.
13	38	8 0	40	52	29,20	67	0,055	SW	2	Cloudy.
	44	2 0	43	55	29,18	68		SSW	2	Fair.
14	33	8 0	33	52	29,41	70	0,022	WNW	1	Fair.
	41	2 0	41	55	29,41	66		SSW	2	Fine.
15	39	8 0	40	52	28,76	68	0,245	SSW	2	Fair. [much wind last night.
	42	2 0	40	53	28,80	68		S	2	Cloudy.
16	34	8 0	35	51	28,83	70		S	1	Cloudy.
	41	2 0	40	53	28,85	70		SSE	2	Cloudy.

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for December, 1809.

1809	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Dec. 17	37	8 0	39	51	28.79	70		WSW	2	Cloudy.
	47	2 0	43	53	28,62	72		ESE	2	Rain.
18	39	8 0	40	50	28,26	73	0,350	SW	2	Cloudy.
	42	2 0	42	53	28,52	70		W	2	Cloudy.
19	40	8 6	40	50	29,22	70	0,025	WNW	2	Cloudy.
	45	2 6	45	53	29.37	68		NW	2	Cloudy.
20	39	8 0	39	50	29.77	68		NW	1	Cloudy.
	44	2 0	43	53	29.74	67		SW	1	Cloudy.
21	35	8 0	35	50	29.74	72	0,080	SW	1	Cloudy.
	44	2 0	44	53	29.77	68		W	1	Fair.
22	39	8 0	40	51	29.96	72		SW	1	Cloudy.
	46	2 0	46	53	29.94	73		SSW	1	Cloudy.
23	38	8 0	38	51	29.93	74		W	1	Foggy.
	40	2 0	40	53	29.93	74		WSW	1	Foggy.
24	32	8 0	34	50	29,82	71	0,040	WNW	1	Snow.
	39	2 0	38	52	29.85	72		NE	2	Sleet.
25	32	8 0	36	48	30,03	72		W	1	Foggy.
	44	2 0	43	52	29.92	72		W	1	Cloudy.
26	40	8 0	40	49	29.77	75	0,140	NE	1	Rain.
	44	2 0	41	52	29.74	74		SW	1	Rain.
27	40	8 0	42	50	29,81	73	0,093	NE	1	Cloudy.
	42	2 0	42	53	29.86	75		NE	1	Cloudy.
28	37	8 0	37	50	30,08	73		NE	1	Cloudy.
	39	2 0	39	52	30,10	69		SW	1	Fair.
29	36	8 0	45	50	29,86	75	0,020	SW	2	Rain.
	49	2 0	49	53	29,60	75		SW	2	Cloudy.
30	43	8 0	43	51	29,72	65	0,020	WNW	1	Cloudy.
	49	2 0	47	53	29,70	66		WSW	1	Cloudy.
31	45	8 0	48	52	29,90	72		W	1	Cloudy.
	52	2 0	52	53	29.95	68		SW	1	Cloudy.

1809.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	55	20	37,1	55	20	37,9	55	42	48,8	30,12	28,50	29,52	76	60	69,7	3,232
February	55	34	45,9	55	34	46,5	59	52	55,3	30,46	28,76	29,78	73	54	64,6	1,603
March	62	33	44,7	61	33	44,8	61	51	55,3	30,47	29,18	30,04	70	54	62,4	0,735
April	57	29	43,8	56	31	44,1	58	50	53,5	30,34	29,06	29,77	72	52	61,9	2,975
May	78	36	58,3	78	40	59,6	69	54	62,5	30,34	29,28	29,90	68	47	56,6	0,812
June	78	45	60,2	78	50	61,2	68	58	62,3	30,44	29,30	29,92	66	46	56,6	1,105
July	78	50	62,4	70	51	62,9	68	61	64,4	30,22	29,44	29,84	74	46	60,6	2,763
August	79	49	61,2	79	51	63,1	69	62	64,4	30,09	29,34	29,75	74	49	61,1	1,427
September	72	40	58,6	72	40	58,7	68	57	62,8	30,16	29,26	29,72	77	52	64,0	2,496
October	65	35	51,8	65	36	52,1	63	53	58,3	30,34	29,85	30,14	77	52	66,3	0,138
November	54	29	41,4	53	47	41,7	58	47	53,1	30,46	29,10	29,93	75	56	67,9	1,092
December	54	32	42,2	52	33	42,6	57	48	52,3	30,24	28,26	29,56	76	63	70,2	2,333
Whole year			50,6			51,3			57,7			29,82			63,5	20,711

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.